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AND

THE ARTS.

VOL. VII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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THE Authors of Original Papers in the present Volume, are Joseph Huddart, Esq. F. R. S.; John Dalton; Everard Home, Esq. F. R. S.; Mr. John Gough; Robert Briggs, M. D.; Mr. Ezekiel Walker; Mr. John Farcy; Mr. Frederick Accum; Sir A. N. Edelcrantz; Richard Chenevix, Esq. F. R. S. and M. R. I. A.; Mr. R. Winter; Mr. James Stodart; Mr. T. Jones; W. H. Woollaston, M. D. F. R. S.; J. M. Elliott; Mr. William Close; Mr. W. Jones, F. Am. P. S.; C. Wilkinson, Esq.; Mr. J. C. Hornblower; Mr. J. J. Hawkins; I. R. I.; Mr. Sharples; B.

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Of English Works, abridged or extracted; Rev. Richard Yates, F. R. S.; Mr. Tho. Willis; John Playfair, F. R. S.; S. H. C. Englefield, Bart. F. R. S.; William Herschel, L. L. D. F. R. S.; T. S. Dyot Bucknal, Esq. M. P.; Benjamin Smith Barton, M. D.; John Stockwell, Esq.; Mr. John Cowie.

Of Engravings the Subjects are, 1. Station Pointer for determining the precise Situation of a Ship or Vessel from two Horizontal Angles, by Joseph Huddart, Esq. F. R. S. 2. Mr. Dalton's Scheme for shewing the Distribution of the Particles in Mixed Gases. 3. Diagram to shew the Method of constructing and computing the Situation of an Observer from

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Soho Square, April, 1804.

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JANUARY, 1804.

ARTICLE I.

Description and Use of the Station Pointer; an Instrument for readily ascertaining the Situation of the Observer after having determined the angular Position of three known Objects. Constructed by JOSEPH HUDDART, *Esq. F. R. S. and communicated by him to the Editor.*

Instrument for giving a ship's place from obs. of two angles of azimuth only.

I HAVE long been desirous of presenting my readers with a drawing and account of the instrument which forms the subject of the present paper. Its utility in protracting the situation of a vessel upon a chart from an accurate observation by the sextant of the angular position of three known objects on shore, instead of the uncertain and rough method of bearings by compass, is great and obvious; and if any other recommendation could be desired, it would be that of its repeated application in the hands of a man of science and experience. This we have the satisfaction to possess in the public labours of Mr. Huddart, whose maritime surveys are too well known and esteemed to require the suffrage of any individual at this day. The very first occasion, when I happened to have the pleasure of conversing with him, I requested the communication of this instrument, with which he with great readiness favouredme, and from which the drawing exhibited in *Plate* I. was made.

Deſcription. It is a protractor with three radii, which being ſet and duly placed on a chart gives the ſtation of the obſerver.

It conſiſts of a circle divided into 360° and filled with three radii, C B, C D and C A; the firſt of which is fixed at zero and the other two are moveable, and have each a nonius ſhewing ſingle minutes of a degree. The centre at C is perforated in order to admit the pointer L, which is cylindrical and well fitted to the hole, where it ſerves to give a correſpondent mark upon the chart or paper when in uſe. I is a magnifier carried by an arm, having its reflector K to throw light on the diviſions, and its cylindrical ſtem H to be inſerted in the hollow centre at C. As it muſt, while in this ſituation, prevent the introduction of the ſteel point L, the ſtem H is itſelf perforated in order to allow a ſmaller ſteel point M to be introduced for the ſame purpoſe. The radii A B and D are of braſs; but they may be prolonged to an extent that would render them heavy and inconvenient if of metal, by the addition of three rulers of wood, which are attached when required by milled head ſcrews and ſteady pins, as ſhewn by breaking the radius A, and alſo in the ſide view at G.

The arms D and B are capable of being brought very near each other, and conſequently of being ſet to a very ſmall angle; but A cannot, on account of the noniuſes, be brought within ten degrees of D. Whenever, therefore, the left hand angle happens to be ſmall, it becomes neceſſary to carry the arm A round, until by its ſituation on the other ſide of B, it comes to be employed to take the right hand angle, and conſequently leaves the left to be taken between D and B.

The pointer A is provided with a clamp, by means of which, that arm can be firſt ſecured in its place, and the pointer D is then ſet without fear of diſturbing the ſituation of the arm firſt ſet. It ſeldom happens that both angles are ſmall in maritime ſurveying; ſo that the condition that A cannot be brought very near either of the other arms, has not been found productive of inconvenience.

In *Fig.* 1. *Plate* III. let A, G, and M repreſent three objects, of which the actual diſtances from each other are known and projected in a chart. Suppoſe an obſerver at H to have meaſured the angle A H M=22°.40′, and the angle M H G = 53°.59′ it is required to protract the place H by means of the ſtation pointer.

Solution. Set the arms of the inſtrument to the reſpective angles, and apply them to the chart ſo that each point A, M, and

and G, ſhall be found upon the fiducial edge of its reſpective pointer. The center will then be found at H, which may be marked by the ſteel point. Deſcription and uſe of the ſtation pointer.

Rationale of the Proceſs. When an obſerver at H ſees M and A at the angular diſtance of 22°.40′ from each other, he muſt himſelf (by Euclid III. 20.21.) be placed ſomewhere in a circle M A H, in which the line A M is the chord of twice that angle. And ſo likewiſe when he obſerves M and G under the angle 53°. 59′ he muſt be placed in another circle M G H, of which M G is the chord of twice that angle. Conſequently H muſt be at a point common to both circles; namely where they interſect. The more direct the interſection the leſs will the reſult be affected by the errors of obſervation:—when the interſections are very oblique, the reſult will in practice become uncertain; and in the caſe where the two circles coincide throughout, the determination by two angles will be no better than by one; that is to ſay H may be any where in that circle. This laſt caſe occurs when a circle drawn through all the objects does alſo paſs through the ſtation itſelf.

In the figure already referred to H, denotes the weſt garret window of my houſe in Soho-ſquare, and the letters A, G, and M reſpectively denote the ſteeples of the pariſh churches of St. Anne Soho *, St. Giles, and St. Martin, laid down by their difference of latitude and departure from St. Paul's church, obtained from the bearings and diſtances in General Roy's Survey's in the Philoſophical Tranſactions, or the *Account of Operations carried on for accompliſhing a Trigonometrical Survey of England and Wales, &c. publiſhed in quarto by Captain Mudge and Mr. James Dalby. London*, 1799, *with 22 plates.*

The table is at pages 194, 195 of the laſt mentioned work. Whence and by obſervation with Hadley's Sextant, the data are: From St. Paul's

St. Anne's bears S. 86°. 9′. 59″ W. 7754 feet.—Southing 518.4—Weſting 7736.6 feet.

St. Martin's S. 74°. 28′. 59″ W. 6748 feet.—Southing 1805.2.—Weſting 6502.1 feet.

St. Giles S. 94°. 36′ 28″ W. 6917 feet.—Northing 55.6—Weſting 6894.7 feet.

* The ſteeple of St. Anne's church has been ſince rebuilt, and ſtands in the ſame latitude, but nine inches farther to the Eaſt than before.

Deſcription and uſe of the ſtation pointer.

Angle obſerved between St. Giles's and St. Martin's from Soho Square 22°. 40′½.

Angle obſerved between St. Martin's and St. Anne's from ditto 53°. 59′.

Conſtruction. Draw the parallel of St. Paul's, and ſet off the points A, G, and M by the reſpective differences of latitude and of meridians to repreſent the churches of St. Anne, St. Giles and St. Martin. Join A and G, the two objects moſt remote in their bearings, and from the extremities A and G on the ſide of the line fartheſt from H, draw the lines Ah, Gh, making angles with A G reſpectively, equal to the angles obſerved on contrary ſides of the line pointing to the middle object M. Through A,h and G deſcribe a circle; and through M and h draw a right line, which prolonged will cut the circle in H. Join A H, G H, and the angles h A G, M H G, and A G h, M H A will be reſpectively equal, becauſe ſtanding on the ſame arcs h G and h A; that is to ſay, the objects will be ſeen from H under the obſerved angles, and conſequently H will be the place of the houſe.

Computation. The numbers in the figure were had by careful conſtruction on a ſcale of one inch to equal 100 feet, uſing a beam compaſs, which divides the inch into 1000 parts. But as the computation may not be unacceptable to beginners in trigonometry, I will here give the proceſs as a concluſion to the preſent paper.

1. The triangle M A G is known. We have therefore the ſide A G and two angles given in the triangle A h G, which is thus determined.

2. In the known triangle M A G by ſubducting the angle A G h from the angle A G M we gain the angle h G M which, with h G, G M, determines the triangle h G M.

3. The angle H h G is (by Euclid I. 32) equal to the ſum of the interior oppoſite angles h G M, G M h. Therefore we have the ſide h G (by paragraph 1) and the two angles H and h in the triangle H h G. Conſequently that triangle is known, and the diſtance from H, the houſe, to the object G may be had.

4. In the triangle M G H the angle at G is equal to the ſum of the known angles M G h, h G H and the other angle at H and alſo the ſide M G are known. Whence the triangle is determined, and the diſtance from H to the object M may be had.

5. In

5. In the triangle M A H, the angle at M is equal to the difference of the known angles G M A, G M H; and the angle at H, together with the two ſides A M, H M are alſo known. Whence the third ſide H A or diſtance from the houſe to the object A may be found.

6. Laſtly the bearing of any of the three ſides of the triangle M A G may be had from the known difference of latitude and meridianal diſtance of its extremities: And this bearing may with eaſe be then applied to ſhew the bearings of the lines H A, H M, H G.

W. N.

II.

Experimental Eſſays on the Conſtitution of mixed Gaſes; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expanſion of Gaſes by Heat. By JOHN DALTON.

(*Concluded from Page* 273, Vol. VI.)

ESSAY III.

ON EVAPORATION.

WHEN a liquid is expoſed to the air it is gradually diſſipated in it. The proceſs by which this effect is produced we call evaporation. Evaporation defined.

Many philoſophers concur in the theory of chemical ſolution: atmoſpheric air, it is ſaid, has an affinity for water; it is a menſtruum in which water is ſoluble to a certain degree. It is allowed notwithſtanding by all, that each liquid is convertible into an elaſtic vapour in vacuo, which can ſubſiſt independently in any temperature; but as the utmoſt forces of theſe vapours are inferior to the preſſure of the atmoſphere in ordinary temperatures, they are ſuppoſed to be incapable of exiſting in it in the ſame way as they do in a torricellian vacuum: hence the notion of affinity is induced. According to this theory of evaporation, atmoſpheric air (and every other ſpecies of air for aught that appears) diſſolves water, alkohol, ether, acids, and even metals. Water below 212° is chemically combined with the gaſes; above 212° it aſſumes a new form, Theory which aſcribes evaporation to chemical ſolution in air—

and

and becomes a diſtinct elaſtic fluid, called *ſteam:* whether water firſt chemically combined with air, and then heated above 212°, is detached from the air or remains with it, the advocates of the theory have not determined.—This theory has always been conſidered as complex and attended with difficulties; ſo much that M. Pictet and others have rejected it, and adopted that which admits of diſtinct elaſtic vapours in the atmoſphere at all temperatures, uncombined with either of the principal conſtituent gaſes; as being much more ſimple and eaſy of explication than the other; though they do not remove the grand objection to it, ariſing from atmoſpheric preſſure. It has however been made to appear in theſe eſſays, I preſume, that the objection to it from preſſure, is itſelf founded upon an ungrounded hypotheſis.

—is obſcure and difficult.

Theory of diſtinct elaſtic vapours in the atmoſphere.

Leaving the theory of evaporation for the preſent, we ſhall proceed to the experiments.

The following poſitions have been eſtabliſhed by others, and need therefore only to be mentioned here.

Poſitions. Different fluids evaporate with different rapidity. Evaporation is as the ſurface; it increaſes with the temperature; and when the air is in motion, and is drier.

1. Some fluids evaporate much more quickly than others.

2. The quantity evaporated is in direct proportion to the ſurface expoſed, all other circumſtances alike.

3. An increaſe of temperature in the liquid is attended with an increaſe of evaporation, not directly proportionable.

4. Evaporation is greater where there is a ſtream of air than where the air is ſtagnant.

5. Evaporation from water is greater the leſs the humidity previouſly exiſting in the atmoſphere, all other circumſtances the ſame.

Objects of the preſent eſſay.

The objects in view in this eſſay, are,

1. To determine the preciſe effect that a variation of temperature has upon the quantity evaporated.

2. To determine the ratio of evaporability of different fluids.

3. To find a rule by which the quantity and effect of previous humidity in the air may be aſcertained.

4. From theſe and other facts to obtain a true theory of evaporation.

On the Evaporation of Water at 212°

Water boiled in a veſſel for a definite time loſt by evaporation a quantity,

I took a ſmall cylindrical veſſel of tin, its diameter $3\frac{1}{4}$ and depth $2\frac{1}{2}$ inches; and having fixed three pieces of wire to equidiſtant points of the circumference, they were faſtened toge-

ther

ther at the top, and the extremities bent into a hook, by which the veſſel might be ſuſpended from the end of a balance, &c. This done, the veſſel was nearly filled with water, which was then made to boil over a ſmall red fire in different circumſtances: it was held in the hand and removed nearer to or further from the fire, ſo as to be kept juſt at the point of ebullition. In this ſtate the veſſel and water were weighed true to a grain, and the inſtant of time noted by a watch; then kept as above at 212° for ten minutes or more and again weighed: and the loſs of water by evaporation, per minute, was thus aſcertained. The experiments were repeated ſeveral times in the ſame as well as in different circumſtances; and the reſults in no inſtance differed materially when obtained in the ſame circumſtances.

The leaſt evaporation per minute was 30 grains: this was when the fire, or lamp, was in the middle of a room, the doors and windows ſhut, and the air calm. —which was leaſt when the ſurrounding air was ſtilleſt,

The next degree was 35 grains per minute or thereabouts: this was when the evaporating veſſel was over a ſmall fire in the uſual fire-place; there being a moderate draught of air, and the room cloſe. —and gradually more,

A briſker fire, cauſing a ſtronger current of air up the chimney, gave from 35 to 40 grains per minute. —the more quickly the air was changed.

When the windows of the room were open, and a ſtrong wind prevailed, the draught over the fire was proportionally increaſed, and the evaporation was from 40 to 45 grains per minute.

The extremes that have thus been noticed are 30 and 45 grains per minute: but were the experiment tried in the open air in high winds, I am inclined to believe from a compariſon of the obſervations, that an evaporation of 50, 55 or even 60 grains per minute might be obſerved.

On the Evaporation of Water below 212°.

I have frequently tried the evaporation at all the temperatures below 212°: it would be tedious to enter into detail of all the experiments, but ſhall give the reſults at ſome remarkable points. In all the high temperatures I uſed the veſſel above mentioned, keeping a thermometer in it, by which I could ſecure a conſtant heat, or at leaſt keep it oſcillating within narrow limits. Water at temperatures below 212° loſt by evaporation.—

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The evaporation from water of 180° was from 18 to 22 grains per minute, according to circumſtances; or about half of that at 212°.

Quantities which were,

At 164° it was about one third of the quantity at the boiling temperature; or from 10 to 16 grains per minute.

At 152° it was only one fourth of that at boiling; or from 8 to 12 grains, according to circumſtances.

The temperature of 144° afford $\frac{1}{5}$ of the effect at boiling; 138° gave $\frac{1}{6}$, &c.

Having previouſly to theſe experiments determined the force of aqueous vapour at all the temperatures under 212°, I was naturally led to examine whether the quantity of water evaporated in a given time bore any proportion to the force of vapour of the ſame temperature, and was agreeably ſurpriſed to find that they exactly correſponded in every part of the thermometric ſcale; thus the forces of vapour at 212°, 180°, 164°, 152°, 144°, and 138° are equal to 30, 15, 10, $7\frac{1}{2}$, 6 and 5 inches of mercury reſpectively, and the grains of water evaporated per minute in thoſe temperatures were 30, 15, 10, $7\frac{1}{2}$, 6 and 5 alſo; or numbers proportional to theſe. Indeed it ſhould be ſo from the eſtabliſhed law of mechanics, that all effects are proportional to the cauſes producing them. The atmoſphere, it ſhould ſeem, obſtructs the diffuſion of vapour, which would otherwiſe be almoſt inſtantaneous, as in vacuo; but this obſtruction is overcome in proportion to the force of the vapour. The obſtruction however cannot ariſe from the *weight* of the atmoſphere, as has till now been ſuppoſed; for then it would effectually prevent any vapour from ariſing under 212°: but it is cauſed by the *vis inertiæ* of the particles of air; and is ſimilar to that which a ſtream of water meets with in deſcending amongſt pebbles.

—in every part of the ſcale proportional to the force of vapour at that temperature.

In low temperatures the evaporation is as the force of vapour at the temperature of the water diminiſhed by that exiſting in the atmoſphere.

The theory of evaporation being thus manifeſted from experiments in high temperatures, I found that if it was to be verified by experiments in low temperatures, regard muſt be had to the force of vapour actually exiſting in the atmoſphere at the time. For inſtance, if water of 59° were the ſubject, the force of vapour of that temperature is $\frac{1}{60}$ of the force at 212°, and one might expect the quantity of evaporation $\frac{1}{60}$ alſo; but if it ſhould happen, as it ſometimes does in ſummer, that an aqueous atmoſphere to that amount does already exiſt, the evaporation, inſtead of being $\frac{1}{60}$ of that from boiling water, would

would be nothing at all. On the other hand, if the aqueous atmoſphere were leſs than that, ſuppoſe half of it, correſponding to 39° of heat, then the effective evaporating force would be $\frac{1}{1\frac{1}{2}0}$ of that from boiling water; in ſhort, the evaporating force muſt be univerſally equal to that of the temperature of the water, diminiſhed by that already exiſting in the atmoſphere. In order to find the force of the aqueous atmoſphere I uſually take a tall cylindrical glaſs jar, dry on the outſide, and fill it with cold ſpring water freſh from the well; if dew be immediately formed on the outſide, I pour the water out, let it ſtand a while to increaſe in heat, dry the outſide of the glaſs well with a linen cloth, and then pour the water in again; this operation is to be continued till dew ceaſes to be formed, and then the temperature of the water muſt be obſerved; and oppoſite to it in the table (p. 264, Vol. VI.) will be found the force of vapour in the atmoſphere. This muſt be done in the open air, or at a window; becauſe the air within is generally more humid than that without. Spring water is generally about 50°, and will moſtly anſwer the purpoſe the three hotteſt months in the year: in other ſeaſons an artificial cold mixture is required.—The accuracy of the reſult obtained this way I think ſcarcely needs to be inſiſted upon. Glaſs, and all other hard ſmooth ſubſtances I have tried, when cooled to a degree below what the ſurrounding aqueous vapour can ſupport, cauſe it to be condenſed on their ſurfaces into water. The degree of cold is uſually from 1 to 10° below the mean heat of the 24 hours; in ſummer I have often obſerved the point as high as 58° or 59°, correſponding to half an inch of mercury in force, and once or twice have ſeen it at 62°: in changeable and windy weather it is liable to conſiderable fluctuation; but this is not the place to enlarge upon it.

Experiment to determine the force of the atmoſpheric vapour.

It is the ſame as that of water, which is at the loweſt temperature, which condenſes no dew on its containing veſſel.

For the purpoſe of obſerving the evaporation in atmoſpheric temperatures I got two light tin veſſels, the one ſix inches in diameter, and half an inch deep, the other eight inches diameter and $\frac{3}{4}$ inch deep; and made to be ſuſpended from a balance, like the former one. When any experiment deſigned as a teſt of the theory was made, a quantity of water was put into one of theſe (generally the ſix inch one, which I preferred) the whole was weighed to a grain; then it was placed in an open window or other expoſed ſituation for ten or fifteen minutes, and again weighed to aſcertain the loſs by evaporation;

Narrative of the experiments of evaporation in atmoſpheric temperatures.

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at the ſame time the temperature of the water was obſerved, the force of the aqueous atmoſphere aſcertained as above, and the ſtrength of the current of air noticed. From a great variety of experiments made both in the winter and ſummer, and when the evaporating force was ſtrong and weak, I have found the reſults, entirely conformable with the above theory. The ſame quantity is evaporated with the ſame evaporating force thus determined, whatever be the temperature of the air, as near as can be judged; but with the ſame evaporating force, a ſtrong wind will double the effect produced in a ſtill atmoſphere. Thus, if the aqueous atmoſphere be correſpondent to 40° of temperature and the air be 60°, the evaporation is the ſame as if the aqueous atmoſphere were at 60° of temperature and the air 72°; and in a calm air the evaporation from a veſſel of ſix inches in diameter in ſuch circumſtances would be about .9 of a grain per minute, and about 1.8 grains per minute in a very ſtrong wind; the different intermediate quantities being regulated ſolely by the force of the wind.

Account of the table of evaporation.

The following table exhibits the ratios and quantity of water evaporated in each temperature, derived from the preceding theory, and confirmed by experiments, as far as they have been extended. The firſt column expreſſes the temperature; the ſecond, the correſponding force of vapour taken from the preceding table; the other three columns give the number of grains of water that would be evaporated from a ſurface of ſix inches in diameter in the reſpective temperatures, on the ſuppoſition of there being previouſly no aqueous vapour in the atmoſphere. Theſe columns preſent the extremes and the mean of evaporation, likely to be noticed, or nearly ſuch: for, the firſt is calculated upon the ſuppoſition of 35 grains loſs per minute from the veſſel of $3\frac{1}{4}$ inches in diameter; the ſecond, 45 and the third 55 grains per minute.

TABLE

TABLE

Shewing the force of vapour, and the full evaporating force of every degree of temperature from 20° to 85°, expreſſed in grains of water that would be raiſed per minute from a veſſel of ſix inches in diameter, ſuppoſing there were no vapour already in the atmoſphere.

Table of the quantities of water evaporated per minute at temperatures between 20° and 85°.

Temperature.	Force of Vap. inch.	Evaporating Force in Grains.		
212°	30	120	154	189
20°	.129	.52	.67	.82
21	.134	.54	.69	.85
22	.139	.56	.71	.88
23	.144	.58	.73	.91
24	.150	.60	.77	.94
25	.156	.62	.79	.97
26	.162	.65	.82	1. 02
27	.168	.67	.86	1. 05
28	.174	.70	.90	1. 10
29	.180	.72	.93	1. 13
30	.186	.74	.95	1. 17
31	.193	.77	.99	1. 21
32	.200	.80	1. 03	1. 26
33	.207	.83	1. 07	1. 30
34	.214	.86	1. 11	1. 35
35	.221	.90	1. 14	1. 39
36	.229	.92	1. 18	1. 45
37	.237	.95	1. 22	1. 49
38	.245	.98	1. 26	1. 54
39	.254	1. 02	1. 31	1. 60
40	.263	1. 05	1. 35	1. 65
41	.273	1. 09	1. 40	1. 71
42	.283	1. 13	1. 45	1. 78
43	.294	1. 18	1. 51	1. 85
44	.305	1. 22	1. 57	1. 92
45	.316	1. 26	1. 62	1. 99
46	.327	1. 31	1. 68	2. 06
47	.339	1. 36	1. 75	2. 13
48	.351	1. 40	1. 80	2. 20
49	.363	1. 45	1. 86	2. 28
50	.375	1. 50	1. 92	2. 36
51	.388	1. 55	1. 99	2. 44
52	.401	1. 60	2. 06	2. 51
53	.415	1. 66	2. 13	2. 61
54	.429	1. 71	2. 20	2. 69
55	.443	1. 77	2. 28	2. 78
56	.458	1. 83	2. 35	2. 88
57	.474	1. 90	2. 43	2. 98

Table continued.

Table of the quantities of water evaporated per minute at temperatures between 20° and 85°.

Temperature. 212°	Force of Vap. inch. 30	Evaporating Force in Grains. 120	154	189
58°	.490	1. 96	2. 52	3. 05
59	.507	2. 03	2. 61	3. 19
60	.524	2. 10	2. 70	3. 30
61	.542	2. 17	2. 79	3. 41
62	.560	2. 24	2. 88	3. 52
63	.578	2. 31	2. 97	3. 63
64	.597	2. 39	3. 07	3. 76
65	.616	2. 46	3. 16	3. 87
66	.635	2. 54	3. 27	3. 99
67	.655	2. 62	3. 37	4. 12
68	.676	2. 70	3. 47	4. 24
69	.698	2. 79	3. 59	4. 38
70	.721	2. 88	3. 70	4. 53
71	.745	2. 98	3. 83	4. 68
72	.770	3. 08	3. 96	4. 84
73	.796	3. 18	4. 09	5. 00
74	.823	3. 29	4. 23	5. 17
75	.851	3. 40	4. 37	5. 34
76	.880	3. 52	4. 52	5. 53
77	.910	3. 65	4. 68	5. 72
78	.940	3. 76	4. 83	5. 91
79	.971	3. 88	4. 99	6. 10
80	1. 00	4. 00	5. 14	6. 29.
81	1. 04	4. 16	5. 35	6. 54
82	1. 07	4. 28	5. 50	6. 73
83	1. 10	4. 40	5. 66	6. 91
84	1. 14	4. 56	5. 86	7. 17
85	1. 17	4. 68	6. 07	7. 46

Uſe of the table familiarly illuſtrated.

The uſe of this table will appear from the following problems:

PROBLEM I.

Having given the temperature at which the aqueous atmoſphere begins to be condenſed into water, and the temperature of the air, to find the quantity of water that would be evaporated in a minute from a veſſel of ſix inches diameter.

Solution. Subtract the grains oppoſite to the lower temperature from thoſe oppoſite to the higher one, in the firſt, ſecond or third column of grains, according to the ſtrength of the wind, and the remainder will be the quantity evaporated in a minute under thoſe circumſtances, nearly.

Example.

Example. Let the point of condenſation be 52°, the temperature of the air 65°, with a moderate breeze. Uſe of the table familiarly illuſtrated.

The number oppoſite 52° in the ſecond column of grains is 2.06, and that oppoſite 65°is 3.16; the difference, 1.1 grain, is the evaporation per minute.

PROBLEM II.

Having given the quantity evaporated in a minute, found by experiment, and the temperature of the air, to find the force of the aqueous atmoſphere, and the point of condenſation.

Solution. Subtract the obſerved evaporation from that oppoſite the given temperature in the table; and look above for the number neareſt to the remainder in the ſame column of evaporation, oppoſite to which will be found the force of the aqueous atmoſphere, and the point at which it begins to be condenſed.

Example. Finding the evaporation from a veſſel of ſix inches in diameter to be 1. 7 grain per minute with a briſk wind, air 62° ; what is the weight of the aqueous atmoſphere, and the temperature at which it begins to be condenſed into water?

The number oppoſite 62° in the third column of grains is 3.52, being the whole evaporating force at that temperature in a perfectly dry atmoſphere; from which take 1.7 grains, the real evaporating force obſerved, and the remainider, 1.82, correſponds, as per table, to the force .294 inches of mercury, the weight of vapour, and to 43° of temperature.*

Evaporation of Spirits, Ether, &c.

If the law of evaporation above given apply to water in every part of the ſcale of heat, no reaſonable doubt can be entertained reſpecting its application to other liquids. I have notwithſtanding made ſeveral experiments on others, the reſults of which are conformable to the ſame law. Some of them follow :— Experiments which ſhew that the evaporation of ſpirits, ether, &c. follow the ſame law as water.

* It may be proper to remind the reader that all the experiments on evaporation are underſtood to be made in the open air, or in a window with a current inward ; alſo it may be obſerved the evaporation in a cloſe room is much leſs and is beſides irregular, being greater proportionably from a leſs ſurface, evidently from the ſtagnation of the air.

1. Spirit

Experiments which ſhew that the evaporation of ſpirits, ether, &c. follow the ſame law as water.

1. Spirit of wine.—Evaporated from a ſurface of four inches in diameter, 54 grains in 25 minutes: air 53°; aqueous atmoſphere at 49°, and beginning to rain with a moderate breeze. It would proportionally have been 121 grains from a veſſel of ſix inches in diameter. This gives nearly five grains per minute. The ſame ſpirit boiled at or near 180°.

Now from the *data*, water of 83° is equivalent in force to ſpirits of 53°: and it may be ſeen that the evaporating force of water of 83° is nearly 5 in the firſt and ſecond columns of grains of the table. It ſeems probable that the aqueous atmoſphere does not diminiſh the evaporation of ſpirits as it does that of water.

2. Ether.—1. Put a phial containing ether, and a ſmall tin veſſel of $1\frac{3}{4}$ inch diameter into a ſcale and balanced them exactly: then poured the ether into the evaporating veſſel and put the phial into the ſcale again; took out 40 grains from the oppoſite ſcale, and waited till the equilibrium was reſtored: this was in 8 minutes 6 ſeconds. The air was 50°, and the ether at firſt 50°; but it rapidly ſunk, as was found by dipping a very ſmall bulbed thermometer into it, to 28°. In a window with a moderate breeze.

——— 2 and 3. Repeated the experiment in the ſame circumſtances, except the evaporating veſſel, which was now porcelain, and $2\frac{1}{2}$ inches diameter. Loſt 40 grains in 3 minutes. Thermometer ſunk from 50 to 30°. The two experiments made this way did not differ above one or two grains.

Theſe reſults reduced to a veſſel of $3\frac{1}{4}$ inches in diameter give

1ſt. Experiment, loſs 17 grains per minute;

2 & 3 ——— — $22\frac{1}{2}$ ———

The reaſon why the reſult in the firſt experiment was ſomething leſs than in the other two, was evidently owing to the circumſtance of its longer duration, by which the ether was the greater part of the time in a low temperature, and conſequently evaporated leſs. The ether uſed boiled at 102°. At 50° it was therefore in the capacity of water at 160°. But water at 160°, at moſt loſes only 17 or 18 grains per minute, and leſs 20° below that temperature. At firſt view therefore it ſhould ſeem that ether evaporates quicker than the general law aſſigns. But it muſt be allowed that the temperature of the *air* has ſome effect upon evaporation, though it has certainly very

very little, Now ether in the above experiments is acted upon by a current of air of an equal or higher temperature than itself; but water of 160° is usually acted upon by air 100° lower than itself, which is every moment precipitating the vapour formed, and thus obstructing its circulation. This appears to be a sufficient cause for the small difference observed.

With respect to mercury, sulphuric acid, muriate of lime, &c. there can be no doubt but they experience a real evaporation like those above; but it must be very small in proportion as their boiling points are high. And it would be difficult to make experiments upon such of these as have an affinity for aqueous vapour; because their acquisition from the aqueous atmosphere would far exceed their loss by evaporation. Evaporation of mercury, &c.

Since writing the above essay, opportunities have occurred to ascertain whether the evaporation from ice is conformable to the same law as that from water. Every one, who has tried the experiment, admits the fact that ice is evaporable. I have lately made several observations on this subject, the results of which, as far as they go, support the conclusion that the general law of evaporation continues the same below the point of congelation as above it. All the experiments were made in the tin vessel above described of six inches in diameter; a quantity of water was suffered to freeze in it, so as to form a circular cake of ice; the vessel and ice were then weighed together, and exposed in the open air for a certain time, after which being again weighed, the loss was found; the force of the aqueous atmosphere was sometimes determined during the experiment by a mixture of pounded ice and salt, in the manner already described. The evaporation from ice agrees with the preceding law.

	Grs.	H.	Grs.	Wind.	Air.	
Nov. 5. In the night lost	110 in	9 ; or,	.20 per m.	N. E. brisk.	28° to 31°	Experimental results.
—— at 10 A. M. —	25 in	$1\frac{1}{2}$; or,	.33 ——	N. E. mod.	32°	
—29. at 1 P. M. —	24 in	$1\frac{3}{4}$; or,	.23 ——	calm.	31°	
P. M. —	84 in	$9\frac{1}{4}$; or,	.15 ——	———	30°	
—30. in the night —	94 in	9 ; or,	.17 ——	N. E. mod.	31°	
Dec. 19. P. M. —	75 in	8 ; or,	.16 ——	N. E. calm.	26°—28°	
In the night —	33 in	11 ; or,	.05 ——	calm.	29°	
—20. A. M. —	21 in	2 ; or,	.175 ——	W. mod	31°	

Some of these being made in the night, and of long duration, neither the temperature of the air, nor the force of the aqueous atmosphere could be fairly determined: the second experiment

experiment was made under every favourable circumſtance, and the aqueous atmoſphere found at 22°. By problem ii, at page 13, it would have been determined at $21\frac{1}{2}$°, uſing the ſecond column of grains in the table.

On the ſubject of evaporation it may be conſidered as unpardonable not to advert to De Sauſſure's valuable Eſſays on Hygrometry.

Experiments of De Sauſſure compared with the preceding theory.

That excellent philoſopher determined, by a well conceived experiment, that dry air of the temperature of 64° or 66°, imbibed aqueous vapour ſo as to increaſe its elaſticity $\frac{1}{54}$ of the atmoſpheric preſſure; and that a cubic foot of ſuch air required 11 or 12 grains of water to produce the effect. By the table above at page 12 it appears the force of vapour at 61° = .54 = $\frac{1}{54}$ of 29 .5 inches nearly. It is probable this difference is occaſioned in part at leaſt by the want of perfect dryneſs in the air he operated upon, which cauſed the increaſe of elaſticity to be leſs than otherwiſe. It was, I think, unfortunate that he attached ſo much importance to and confidence in his hygrometer; and that he adopted the theory of chymical ſolution of water in air, contrary to the facts he diſcovered, which ſeemed more reconcileable to the notion of aqueous vapour being a diſtinct elaſtic fluid. Indeed he is forced to acknowledge in the 1ſt. chap. of his Eſſay on the Theory of Evaporation, that in the ordinary temperature of the atmoſphere, aqueous vapour is formed in the firſt inſtance a diſtant elaſtic fluid, and *after it has been converted into an elaſtic fluid*, it is diſſolved by the air; "Je crois qu'il ne la diſſout que lorſque l'action du "feu l'a convertie en vapeur elaſtique." Now if it can for a moment exiſt independently under the preſſure of the atmoſphere, why may it not continue to exiſt in that ſtate?

He confided too much in his hygrometer,

His table of the weight of aqueous vapour in a cubic foot of air at different degrees of the thermometer, being derived from experiments with his hygrometer, except the ſtandard one of 66° (15° Reaumur), is far from accurate; and the inaccuracy increaſes with the diſtance from the ſtandard, which, as has been obſerved, appears to be nearly correct: in the higher temperatures he makes the water diſſolved too little, and in the lower temperatures too much. He ſays (§ 93) that the loweſt he has ſeen the hygrometer in the open air, is 40; and that it indicated a reduction of temperature in the air amounting

and on that account his tables are inaccurate.

amounting to 34° .7 (78° of Fahr.) was neceſſary in order to depoſit dew. This obſervation alone is ſufficient to render his hygrometer ſuſpected; for, few who have attended to the formation of dew will admit the probability of ſo large a reduction being neceſſary in any climate or ſeaſon: I believe it rarely requires 40° reduction in temperature in any part of the world to produce the effect.

Engraving to ſhew the conſtitution of the atmoſphere.

Plate I.* is intended to illuſtrate the author's conception of the conſtitution of the atmoſphere. The different marks or characters of the particles of the gaſes are merely arbitrary, and intended for diſtinction; the ſimple atmoſpheres are given nearly on their real denſities, and the particles are arranged at equal diſtances from each other. In the compound atmoſphere the ſame arrangement is made of each kind of particles as in the ſimple; but the particles of different kinds do not arrange at regular diſtances from each other; becauſe it is ſuppoſed they do not repel each other.

III.

Obſervations on the Cultivation and Growth of Oak Timber. In a Letter from the Rev. RICHARD YATES, *F. A. S. Chaplain to His Majeſty's Royal Hoſpital at Chelſea, to* CHARLES TAYLOR, *Eſq. Secretary to the Society of Arts* †.

SIR,

Great importance of the culture of the oak.

TO expatiate upon the vaſt importance of increaſing the growth of oak-timber, ſeems unneceſſary. The national advantages reſulting from this ſource appear to be in general well underſtood; and yet the cultivation and management of this moſt uſeful plant has not hitherto obtained that degree of attention which it moſt certainly merits.

Entirely to obviate, or even in ſome meaſure to remove or leſſen, the obſtacles that ſtill continue to impede the planting

* From the ſame Vol. of the Mancheſter Memoirs.—I remind the reader of the references at page 257 of our laſt.

† For which the ſilver medal was voted. Memoirs for 1802, page 80.

of oaks, would therefore be rendering an eſſential ſervice to the nation. The deſire of accompliſhing ſo beneficial a purpoſe, has induced the judicious and public-ſpirited conductors of the Society of Arts to propoſe a premium for "aſcertaining "the beſt method of raiſing oaks:"—in conſequence of which, this paper is ſubmitted to their candid conſideration. And as the ſtatements here made are founded upon a ſedulous and active experience of fifty years, it is preſumed the *ſpirit* and *meaning* of the Society's propoſal may have been obſerved, although it has not been poſſible (in this inſtance) *literally* to fulfil its terms; at leaſt, the very intention of promoting and forwarding the views of ſo enlightened and highly uſeful a Society, may, it is hoped, be accepted as an apology for calling their attention to theſe obſervations.

Cultivation of the oak diſcouraged,

It forms no part of the preſent deſign to enter minutely into the various cauſes that continue to operate in obſtructing the cultivation of oak; as there is one of peculiar magnitude, the conſequences of which are highly detrimental and injurious, and which it is therefore the principal object of this paper to remove.

from a notion of its very ſlow growth,

An opinion is generally prevalent, that the oak is particularly ſlow in its growth, and requires a great number of years before it affords any advantage. This idea too often deters from planting, on account of the very great length of time it is ſuppoſed the land muſt be occupied before any return of valuable produce can be obtained from it, after a conſiderable expence may have been incurred in forming plantations.

which is a miſtake.

This opinion I conſider as entirely founded in error, and to have taken its riſe in a great meaſure from the want of proper management that has hitherto commonly prevailed in the raiſing of oaks: and in this paper I ſhall endeavour ſtrongly to ſtate, that the oak may be rendered very rapid in its growth, and that conſequently land may be employed to great advantage in its cultivation, as a very conſiderable and profitable produce may, in a much ſhorter time than is generally ſuppoſed, be derived from proper parts of an eſtate thus employed.

Oaks are ſuffered to grow (ſlowly) with a ſhort ſtem and large head: *but their ſize may be*

Oak-timber in this country, for the moſt part, appears in trees of a conſiderable extent of head, but ſeldom more than *twenty* or *thirty* feet in ſtem; and this, in many inſtances, the growth of a century. Now, by the courſe of management here

here propoſed, it is conceived that trees, of at leaſt *double this magnitude*, may be obtained in about half that time. *doubled in half the uſual time.*

It is not my intention to attempt a proof of this propoſition by theoretical deductions, but to appeal for its confirmation to the indubitable teſt of fact, which, from the event of repeated trials, impreſſes a conviction, that experience will be found to ſupport and eſtabliſh it in the moſt unequivocal manner.

It would be eaſy to enlarge much on the various qualities of ſoil, the nature and proceſs of vegetation, and the peculiar properties of the oak; but as theſe topics may be found amply and judiciouſly diſcuſſed in many other authors, who have expreſsly treated on theſe ſubjects, I ſhall decline all ſuch ſpeculations: and, with the hope of being more eſſentially uſeful, ſhall confine myſelf to a ſtatement as ſimple and practical as poſſible.

The oak, in the progreſs of its growth, ſpreads numerous roots near the ſurface of the ground, and in an horizontal direction: theſe aſſiſt in ſupporting and preſerving the tree in its poſition, but ſeem to contribute very little to its increaſe and magnitude. The oak appears to derive its chief nutriment and ſtrength from a root that always deſcends at right angles to the horizon, and is called the tap-root. The firſt thing, therefore, to be obſerved is, that upon a judicious attention to this peculiarity, the planter's ſucceſs principally depends; and the neglect of this care is the conſtant ſource of error and diſappointment. In all climates, and upon all ſoils, to preſerve this tap-root from injury, and as much as poſſible to aſſiſt its growth, is a general, and indeed the moſt eſſential principle in the cultivation of oak. With a due regard to this circumſtance, the management of a plantation may be reſolved into the three following practical directions: It is of the utmoſt importance that the perpendicular or tap root be preſerved.

Previouſly to planting the acorns, *looſen* the earth intended for their reception, by *deep trenching*. Inſtructions for preparing the ground and planting acorns.

Never tranſplant, or in any way diſturb, the ſaplings intended for timber.

Keep the plant carefully *pruned*, till arrived at a proper height.

More fully to elucidate the ſubject, and to prevent the poſſibility of miſapprehenſion, it may be proper to give a more detailed ſtatement.

Oaks for timber are never to be tranſplanted.

In determining on a ſpot to form a plantation of oaks for timber, it muſt always be recollected that the plants are to remain without removal in their firſt ſituation: the clearing and fencing may then be attended to as uſual; and in the courſe of the winter, from September to March, the particular ſpots intended for the reception of acorns, may be prepared for that purpoſe, by digging a trench about three feet in width, and from three to ſix feet in depth, according to the cloſeneſs and tenacity of the ſoil. If graſs-ground, the firſt ſpit ſhould be placed at the bottom of the trench; and if more than one trench be neceſſary, they ſhould be prepared in the ſame manner, preſerving a diſtance of ten yards between each, if it be intended to employ the intermediate ſpace in underwood, or for any other purpoſe.

Having made a careful ſelection of acorns that are perfectly ſound, and in good preſervation, they are to be planted about the middle of March. Draw a drill in the centre of the trench; two inches in depth, if the ſoil be heavy and loamy; but three inches in a light and ſandy earth. In this place the acorns two inches aſunder, and cover them carefully with mould. When the plants appear, they muſt be weeded by hand in the rows, and the earth of the trench round them cleaned with a hoe, once a month during the ſummer. In October inſpect the rows, and thin them by pulling up every other plant: attention will of courſe be paid to remove the weak and crooked plants, and leave thoſe that are talleſt and ſtraiteſt. On the ſecond year, the operation of thinning muſt be repeated, at the ſame time, and in the ſame manner; and, ſhould any of the remaining plants have made ſide-ſhoots ſtronger than the general character, they muſt be ſmoothly cut off with a ſharp knife, cloſe to the leading ſtem. On the third year, the thinning is again to be repeated, and the general pruning commenced, by cutting off cloſe to the leading ſtem all the ſide-ſhoots of the firſt year; thus leaving the branches of two years to form the head of the following year. The removal of every alternate plant muſt be continued yearly, till the trees are about thirty feet apart, at which diſtance they may remain for timber. The pruning is to be continued, by removing every year, very ſmooth and cloſe to the main ſtem, one year's growth of ſide branches, till the plants are arrived at a ſtem of forty, fifty,

or

or ſixty feet, and they may then be permitted to run to head without further pruning. Inſtructions for cultivating the oak.

The particular arrangement here recommended may be varied according to any peculiarities of ſituation, regard being conſtantly had to the general and moſt important principle of looſening the ground *very deep* previouſly to planting the acorns. By this mode of culture, oaks may be raiſed in almoſt any ſoil; but, where it is poſſible, a loam or marl is always to be choſen. Oaks thrive much the beſt in ſuch earth; and, when aſſiſted by *deep trenching* and *judicious pruning*, attain in a few years to an immenſe ſize.

Thoſe who have been accuſtomed to notice the ſlow growth and ſtunted appearance of oak trees, when denied the aſſiſtance of art, and left to themſelves in the common way, would obſerve with aſtoniſhment the vigorous and rapid increaſe of plants under the management now pointed out.

The plants thinned out the firſt three or four years, though not fit to be depended upon for timber, as tranſplanting generally injures very materially the future growth, may be replanted in the intermediate ſpaces between the rows, for the purpoſe of being afterwards removed; or they may be uſefully placed in hedges, or other ſpare and unoccupied ſpots of ground. They ſhould be headed down at the time of tranſplanting, as this operation aſſiſts the proceſs of nature, in reproducing or remedying any injury the tap-root may have received from the removal: and, if proper attention be given to looſening the ſoil for their reception, and pruning them as they advance, in moſt inſtances an adequate profit will be derived from the labour beſtowed upon them. After a few years, the produce of the timber-plantation will be found very advantageous. The young trees that are to be removed yearly, will always find a ready market for a variety of purpoſes unneceſſary here to enumerate. In addition to theſe advantages, if by this treatment of *deep trenching* previous to planting, and *annual careful pruning* during the growth, timber can be produced in about fifty years, of equal quality, and much ſuperior in ſize, to that which has been above one hundred years growing under improper management, or without the aſſiſtance of cultivation; it will doubtleſs be allowed that a moſt beneficial, if not abſolutely the beſt poſſible method of "raiſing oaks," is here pointed out and aſcertained.

This

Inſtructions for cultivating the oak.

This method of cultivation may perhaps be thought to occaſion ſo much expenſe in manual labour as to prevent its being generally adopted: it might perhaps be ſufficient to obſerve, that if the work be conducted with judgment and economy, the future produce would afford ample returns for all neceſſary expenditure: it ſhould alſo be recollected, that the previous preparation of the ground, and the ſubſequent pruning of the plants, are both to be performed at that ſeaſon of the year when a ſcarcity of work will enable the planter to obtain aſſiſtance upon eaſier terms; with this additional advantage alſo, of providing employment for the labourer at thoſe times when the general ſtate of agricultural buſineſs renders it difficult for him to find maintenance for himſelf and family without charitable relief.

In 1750, at Ingeſtrie in Staffordſhire, the ſeat of Lord Chetwynd, ſome plantations were formed and managed in a great meaſure according to the principles here ſtated, and the growth of the plants were ſo uncommonly rapid, and ſo extraordinary, that it could not but attract the notice of all concerned in the conduct of them. The attention to the ſubject, then excited, has been the occaſion and ground of all the obſervations and experiments made from that time to the preſent, the reſult of which is given in this paper.

The extenſive plantations of the late Lord Denbigh, at Newnham Paddox, in Warwickſhire, are well known and much admired. The whole has been conducted with great judgment. About a ſquare acre has been employed in raiſing oaks upon a plan nearly ſimilar to that now propoſed, and affords the beſt and moſt convincing proof of the ſuperior utility and efficacy of ſuch management. Had the noble Earl been now living, I ſhould have been enabled to have laid before the Society ſome more detailed particulars: That, however, is now impoſſible; this paper, therefore, in its preſent ſtate, may perhaps be thought not altogether unworthy of notice, as tending to forward the liberal deſigns of the Society, and contributing to the advantage of the public, the author conceiving that the beſt method of raiſing oaks is aſcertained and ſtated in it.

Should the Society be in any degree inclined to join in this ſentiment, it may perhaps induce them to make ſome alteration in the terms of their propoſal; as, according to the ſtatements made

made in this paper, and indeed from what may be seen in every part of the kingdom, in the character and appearance of oaks growing without cultivation, it seems ascertained, that "acorns set with the spade or dibble, without digging or "tillage," can never be depended on to form good timber; and even in the most favourable circumstances of this case, the growth will be exceedingly slow and precarious. The same may be said of "young plants, previously raised in nurseries, "and transplanted;" for if the tap-root be cut, broken, or in any degree injured, which in transplanting it is almost impossible to avoid, that plant will seldom become a vigorous and flourishing tree. To form a course of experiments on such a plant as the oak, is not a very easy matter. To fulfil explicitly the conditions of the Society would require a great length of time, and would be attended with considerable expense, from which future candidates may in a great measure be exonerated. The raising even one acre in the manner here ascertained might be productive of great pecuniary advantage, if the facts and experience detailed in this paper are permitted to prove the inutility of the other two methods, and consequently to remove the necessity of employing so much ground upon them, at an expense they will never repay.

Instructions for cultivating the oak.

Chelsea College, Nov. 4, 1801.

IV.

A First Memoir on coloured Shadows.

By Cit. J. H. Hassenfratz.

(Concluded from Vol. VI. page 285.)

PART THE SECOND.

THOUGH the shadow produced by the concurrence of the solar light with that of the atmosphere, usually presents only that series of colours contained in the portion of the spectrum, between green and violet, it is not the same with the coloured shadows observed in the interior of rooms, when several lights direct or reflected concur with that of the atmosphere to enlighten the plane on which the shadow is observed

Shadows produced by more than two lights, are more various in their tints than those before described.

obſerved; theſe are capable of ſhewing all the colours of the priſm; in fact, we ſee red, orange, yellow, green, blue, indigo and violet, more or leſs blackened.

Number of ſhadows.

The number of theſe ſhadows is almoſt always two or three, ſometimes four or five; it has even happened that we have diſtinguiſhed ſix.

When theſe ſhadows are two in number, the colours they preſent are always thoſe called *complementary* colours.

Combination of colours to imitate the ſolar ſpectrum.

The infinite variety of colours obſerved in the ſolar ſpectrum may be imperfectly imitated by combining different proportions of the natural colours, red, yellow and blue; and, according to the obſervations of Newton, an artificial white may be formed, by mixing theſe three colours. Thoſe colours are denominated *complementary*, which muſt be made with one or two of the three colours cited, to produce an artificial white with a given colour.

Complementary colours.

To obtain an artificial white with a red, the yellow and blue muſt be mixed with it. The yellow and blue make a green, green is therefore the complementary colour of red, and *vice verſa*.

The complementary colour of the orange formed by yellow and red is the blue.

The complementary colour of yellow is violet.

The term does not ſubvert the Newtonian theory of the ſolar light.

As the complementary colours meet together in a great number of caſes, we have thought it neceſſary in the outſet to give them that denomination, which makes no change in the reſults, by which Newton proves that the ſolar ray is compoſed of an infinity of homogeneous coloured rays.

If the ſpectrum is divided into two parts at the point where the green commences, all the homogeneous rays of the lower part, that is to ſay, from the beginning of the red to the beginning of the green, are complementary to thoſe of the upper part, that is to ſay, from the green to the violet; and that in the natural order of their appearance.

The denomination of complementary colours has no other object but to point out two colours which are found together very frequently; and to avoid, by this ſimple medium, the circumlocution which their diſtinction would lead to.

Complementary colours of two ſhadows;

Whenever two coloured ſhadows are ſeen, they are nearly always complementary to each other; that is to ſay, if the ſhade of one is red, orange, or yellow, that of the other is green, blue, or violet.

When the number of the ſhadows is three, one of them is almoſt always complementary to the other two. of three;

When there are four ſhadows, two of them are ſometimes complementary to the other two; at other times, one of the ſhadows is complementary to the remaining three. of four.

When there are only two coloured ſhadows, one of them is always of the colour of one of the bodies which reflects the light. If the apartment, in which the ſhadows are ſeen, is commanded by a covering of ſlates which reflects a bluiſh light on the plane where the ſhadows fall, the two colours are blue and orange; if the covering is of tiles, the colours are nearly the ſame. If a meadow or trees reflect light into the apartment, the ſhadows are green and red. If the apartment has coloured hangings which reflect the atmoſpheric light or that of the ſun, the ſhadows will participate of the colours of the lights reflected externally and internally. One of the ſhadows is of the ſame colour as the reflected light; which may be varied by another light,

When the light is reflected through glazed windows upon the plane where the ſhadow is ſeen, as the glaſs is rarely colourleſs, and generally tinged with green or violet, according as iron or manganeſe predominates in its compoſition, the ſhadow participates in the colour communicated to the light in its paſſage through the glaſs. or by paſſing through glaſs,

If the ſurface on which the different lights are received be itſelf coloured, the colours of the ſhadow will be affected by it, and the problem will become more complicated. or by the colour of the ſurface on which the ſhadow is received.

From the obſervations we have collected, it follows naturally, that the coloured ſhadows of the interior of an apartment are capable of yielding all the colours of the ſpectrum; that theſe colours are governed by thoſe of the bodies which reflect the light and illuminate the ſurface on which the coloured ſhadows are obſerved. Thus far theſe obſervations offer nothing which could not have been foreſeen by every perſon who had reflected a little on ſhadows; but that which is remarkable is, that theſe ſhadows are almoſt always accompanied by other ſhadows whoſe colours are complementary to the reflected colours, although no body is diſcovered which is capable of reflecting theſe ſorts of colours. Inferences. Other complementary ſhadows not produced by direct reflection.

Here then is a new ſhadow which ſeems conſtantly to accompany the firſt, whoſe colour has a peculiar affinity for that of the light which produces the primitive ſhadow, and depends on cauſes which we purpoſe to examine in a ſubſequent memoir.

Part

Part the Third.

Shadows from two lights are of different colours.

IF in a clear moon-light night, at a time when the lamps are alight in Paris, an opaque black body be brought near a white paper, enlightened both by the moon and a lamp, two diſtinct coloured ſhadows will be perceived, one reddiſh, the other bluiſh.

If a taper, a candle, a lamp, or any other light, be brought near a white paſteboard enlightened by the moon, an opaque body placed at a ſmall diſtance from the paſteboard, forms two coloured ſhadows; that occaſioned by intercepting the light of the moon is reddiſh, and that produced by the other light is blue.

Theſe two ſhadows may be obtained by enlightening a ſurface with the atmoſpheric light and that of a lamp; but this requires that the light of the atmoſphere muſt enter the room where the experiment is made, through a ſmall opening, in order that the ſhadow may be well defined when this light is ſingle.

The effect was not varied by the nature of one combuſtible body made uſe of.

By admitting the light of the atmoſphere into a darkened room through an opening of a decimetre (four inches) in diameter, and illuminating a white paſteboard by this light and that of a lamp, we have found that when the paſteboard was one or two metres (yards) from the opening, two coloured ſhadows were produced; that of the atmoſphere is conſtantly red, and that of the artificial light, blue. We have employed the lights from coals, wood, tapers, candles, alcohol, and even hidrogen gas.

The greateſt part of the ſhadows obtained from two different lights are of two tints, the one reddiſh, the other bluiſh.

The variations in the colour of ſhadows are independent of the intenſity of the lights which illuminate the ſurface. We were careful in all the experiments we are going to relate, to place the light at ſuch diſtances that they ſhould enlighten equally that part of the white paſteboard on which the ſhadows were projected; and to this end we followed the method made uſe of by Bouguer, and which conſiſts in receiving through two holes, on a piece of oiled paper, the rays from two different lights, and removing the ſtrongeſt, or bringing forward the weakeſt, until the two lights were of equal intenſity; always being careful to place the eye at an equal diſtance from the two enlightened points.

The

The experiments were made in a dark room, every part of which was painted black, for the purpoſe of deſtroying the colour produced by reflected light. Precautions in making the experiments.

We at firſt illuminated the paſteboard by the light obtained from the combuſtion of fiſh-oil in a common lamp, and by the light of hidrogen gas, produced by the diſſolution of zinc in ſulphuric acid weakened with water. The ſhadow produced by intercepting the light of the hidrogen gas, was reddiſh; that ariſing from intercepting the light of the lamp, was bluiſh. Shadows from hidrogen gas and oil;

At the beginning we only uſed a feeble light from the hidrogen gas. Being fearful that the colour of the ſhadows might be occaſioned by the weak light, we filled a bladder with the gas, and by compreſſing the bladder ſtrongly, produced a powerful bright light: the plane illuminated by this light and that of the lamp, afforded a ſimilar reſult; the ſhadow from the hidrogen gas was reddiſh, that from the lamp bluiſh.

On enlightening the paſteboard by the light of a lamp and that of alcohol, the interception of the light of alcohol produced a reddiſh ſhadow, and that of the lamp a bluiſh ſhadow. from a lamp and alcohol;

By illuminating it with the light of the lamp and that of a candle, the ſhadow from the candle was black, with a light-reddiſh tinge; that of the lamp was black, with a light-bluiſh tinge. from a lamp and a candle;

The paſteboard being illuminated with the light of a lamp and that of a taper, the ſhadow from the taper was black ſlightly reddened, and that of the lamp, black a little bluiſh. from a lamp and a taper;

The paſteboard illuminated, at the ſame time, by the light of a lamp and that from the combuſtion of wood, the ſhadow thrown by the light of the wood was a violaceous blue; that of the lamp a reddiſh yellow. from a lamp and wood,

The experiment was tried with wood burning with difficulty, and producing little flame; with very dry wood producing much flame; with chips of wood producing a white flame, which reddened that of the lamp. When the ſurface was enlightened with lights of equal intenſity, the coloured ſhadows offered little variation; that proceeding from the light of the wood was violaceous blue, that of the lamp reddiſh yellow. in different ſtates.

The firſt experiment was repeated in glaſs-houſes where wood is burnt. However bright the flame might be which was emitted through the working flues, the ſhadow produced by it on a ſurface alſo illuminated by the light of a lamp, was a violaceous blue, while that from the lamp was reddiſh yellow. The experiment repeated in a glaſs-houſe where wood is burnt;

On

in an iron forge; On illuminating the paſteboard by the light of a lamp and that from the combuſtion of culm in an iron forge, the ſhadow ariſing from the light of the culm was bluiſh, and that from the lamp reddiſh.

in a glaſs-houſe where culm is burnt; On illuminating the paſteboard by the light of a lamp and that from the culm burnt in the furnaces of a glaſs-houſe, where the brilliant flame of the working flues reddened that of a lamp, the reſult was ſimilar to the preceding: the light from the culm produced a blue ſhadow, and that of the lamp a reddiſh ſhadow.

with a lamp and charcoal. When the paſteboard is illuminated by the light of a lamp and that from the combuſtion of charcoal, the ſhadow of the charcoal is blue, and that of the lamp red.

The paſteboard illuminated by the light of a lamp and that of charcoal urged by the large bellows in a chemical furnace, ſo as to produce a whitiſh light as well as that violaceous light ſometimes obſerved in the furnaces of a forge and in tall furnaces, conſtantly afforded the ſame reſult. In theſe two caſes the ſhadow from the light of the charcoal was bluiſh, and that from the lamp reddiſh. The identity of the colours of the ſhadows obtained in theſe three experiments with charcoal, notwithſtanding the colour of the flame was different in each experiment, leads to an opinion that the reſult depends on the nature of the combuſtible. Nevertheleſs, to obtain more probability, we made further obſervations. We filled a chemical furnace with charcoal, and having covered it with its dome for the purpoſe of comparing the ſhadow of the direct light of the violaceous flame ariſing from the combuſtion of the charcoal, with that of the light of a lamp, we took notice of the ſhadows of theſe two lights at the moment when the flame appeared at the ſummit of the furnace. For a conſiderable time we obſerved that the colours of the ſhadows were very uncertain: ſometimes the ſhadow of the lamp was blue, and that of the flame red; at other times the ſhadow of the flame was blue, and that of the lamp red. This alternate change in the colours of the ſhadows from blue to red, continued until the charcoal, which was in the upper part of the dome, was completely inflamed; then the colours of the two ſhadows became fixed, that from the flame of the charcoal became bluiſh, and that from the light of the lamp reddiſh.

Probability that the colour of the ſhadows is produced by the nature of the combuſtible.

Singular change of colour in the ſhadows.

Repetition of the experiment by burning the

As, from the obſervations of ſeveral perſons to whom theſe reſults were communicated, we were induced to believe that the

the great intenſity of the light of charcoal obtained by its combuſtion in oxigen gas, might change or make ſome variation in the colour of the ſhadows, we ignited the extremity of a piece of charcoal, and urged its combuſtion by a jet of oxigen gas; the gas was contained in a bladder, which was preſſed by the arm; the light obtained was extremely brilliant. A white parchment, illuminated at the ſame time by this light and that of a lamp, preſented two coloured ſhadows; that from the light of the charcoal was blue, and that from the lamp red.

charcoal in oxigen gas,

with a ſimilar reſult.

From the experiments we have detailed it reſults, that the ſhadow of the light of hidrogen and of alcohol is reddiſh, when that of the lamp is blue; that the ſhadow of the light of wood, of culm, of charcoal, is bluiſh, and that from the light of the lamp is reddiſh; and as hidrogen and alcohol contain leſs carbon than oil—and dry wood, culm, and charcoal contain leſs hidrogen than oil; it would ſeem that the blue and red colours of the ſhadows of the two artificial lights, bear a relation to each other correſponding with the proportions of theſe two combuſtibles; that the light produced by a ſubſtance in which hidrogen predominates, gives a reddiſh ſhadow, and that produced by a ſubſtance in which carbon predominates, affords a bluiſh ſhadow.

Summary of the experiments;

whence it is inferred that the colour of the ſhadows depends on the predominancy of hidrogen or carbon.

Although our experiments appear to lead to that concluſion, and we took great care to be certain that the red and blue ſhadows produced by the two lights were independent of the rapidity of the combuſtion and intenſity of the lights; nevertheleſs, we dare not hazard the preſenting this reſult as forming a general law; we prefer waiting until time and new experiments ſhall confirm or deſtroy them.

Recapitulation.

FROM the obſervations and experiments related in this memoir, it follows,

1*ſt.* That the ſhadows formed by the direct light of the ſun and that of the atmoſphere, vary from meadow-green to a violet-black, in a gradation through the blue, indigo, and violet; and that this variation depends on the intenſity of the light (of the ſun) compared to that of the atmoſphere.

2*d.* That the ſhadows formed in apartments by the light of the atmoſphere and reflected lights, may preſent all the priſmatic

matic colours, more or leſs changed by black; and that the tints of the ſhadows obſerved are always complementary to each other.

3*d*. That the ſhadows produced on a paſteboard illuminated by artificial lights, are reddiſh and bluiſh, more or leſs deep; and that very probably the bluiſh and reddiſh tints of the ſhadows depend on the proportions of hidrogen and carbon in the combuſtible bodies.

The ſubject to be reſumed.

We purpoſe, if the Inſtitute judges this ſubject worthy of their attention, to make known, in other memoirs, the experiments we have made to determine the different cauſes which contribute to form coloured ſhadows in every particular circumſtance.

V.

Account of the Preparation of the Hyacinthus non ſcriptus, or common Blue-Bell, as a Subſtitute for Gum-Arabic. In a Letter from Mr. Thomas Willis of Lime-Street, London, communicated to Charles Taylor Eſq. Secretary to the Society of Arts *.

SIR,

Communication by Mr. Deyeux reſpecting gum from the hyacinthus non ſcriptus.

I HAVE obſerved in the Appendix of the laſt Monthly Review, in their Report of the Chemical Annals, No. 115, that mention is made of a letter from Mr. Deyeux, to the authors of the Annales de Chimie; in which an account is given that a gummy ſubſtance had been diſcovered, by Mons. Deyeux, to be contained in the bulb of the plant called hyacinthus non ſcriptus; and that the reviewers only ſay, "This "article does not at preſent require any further notice than annunciation." As I know, Sir, your benevolent diſpoſition in promoting and encouraging the arts, the ſciences, and manufactures of this kingdom, I beg leave to offer the following obſervations on the above-mentioned article, which I think a ſubject proper to lay before your moſt excellent Society, and which I have no doubt may become of national utility.

* Extracted from the Society's Memoirs from page 202: The ſilver medal was adjudged to this Communication.

In the year, 1794, whilſt collecting plants in a wood for botanical ſpecimens, I obſerved that the root of the hyacinthus non ſcriptus, the plant commonly called blue-bells, or hare-bells, was extremely mucilaginous; and on taſting it, I diſcovered only a very ſlight pungency. I collected a pound of the bulbs, and, after ſlicing and drying them before a fire, they yielded about four ounces of powder. I thought that by keeping the powder ſome time, the little acridneſs might go off, as it does in the arum-root powder. I taſted it about ſix months after, and found it perfectly inſipid. I concluded it might be rendered uſeful for food or nouriſhment, but at that time purſued the matter no further.

Diſcovery of this mucilage by the author in 1794

In the ſpring of 1800, gum-arabic having been a long time very dear, and likely to continue ſo, I thought this mucilaginous root might anſwer ſome of its purpoſes, for external uſe. I therefore procured ſeven pounds and a half of the bulbs, which, when ſliced and dried, produced two pounds of powder. Being ſoon afterwards in company with Mr. Charles Taylor, Secretary to the Society of Arts, &c. I mentioned to him that I had diſcovered a root which grew in great plenty in this kingdom, yielded a very ſtrong mucilage, and which I imagined would anſwer the purpoſe of gum-arabic, in ſome of the manufactories. He ſaid, if I pleaſed, he would ſend ſome of it down to Mancheſter, to be tried by the calico-printers.

Propoſal in 1800 to uſe it inſtead of gum-arabic.

Three or four ounces of the powder were given him, and ſent down there: he was informed, upon trial, that it anſwered the purpoſes of fixing the calico-printers colours, equally as well as gum-arabic; and in the ſame proportion, of an ounce and a half of the powder, to four ounces of the mordant. Mr. Taylor received the ſamples of the printed cottons on which it had been uſed.

Trial at Mancheſter ſucceſsful.

On the 15th of January, 1801, I furniſhed Mr. Taylor with eight ounces more of the powder; but have not ſince heard the reſult.

As this root can be eaſily procured, and uſed at a leſs price than gum-arabic has been ſold for ſeveral years paſt, I think it may be rendered of great utility; and the Society of Arts, &c. by patronizing it, may be the means of making it a public benefit.

The root is cheap.

Care

Inſtructions for collecting the roots and ſecuring their growth.

Care ſhould be taken, and advice given, that the woods ſhould not be left deſtitute of the roots; and it would be adviſeable to offer premiums for the cultivating the roots and offsets, as they are very increaſing. By ſuch means a conſtant ſupply may be had, if the roots anſwer the intended purpoſes.

I do not preſume to offer any thing reſpecting the mode in which the Society may think proper to divulge the diſcovery, and promote the uſe of theſe roots; but I imagine, that if the roots are bruiſed and uſed freſh, they would anſwer the purpoſe better than when dried and powdered; and as it is now a proper time of the year for taking them up, and will continue to be ſo for two months, I wiſh that the diſcovery may be made known as ſoon as poſſible.

I have ſent you ſpecimens both of the dried roots and powder, that they may be ſeen at the Society's rooms, by the calico-printers. What I have done have been ſcorched a little in drying; but the colour would be much better, if proper care was taken in drying them.

I am, SIR,

Your moſt obedient Servant,

THOMAS WILLIS.

Lime-Street,
March 17, 1802.

To John Baker, Eſq. one of the Members of the Society of Arts, &c.

From the trials made before the committee with this powder, with hot and cold water, from ſamples of the printed cotton produced which had been printed therewith inſtead of gum ſenegal, and from experiments made in Mancheſter, it appears that the hyacinthus non ſcriptus may, in many caſes, be found a uſeful ſubſtitute for gum-arabic.

A Memoir

VI.

A Memoir on Vinous Fermentation: by Cit. THENARD *.

THE vinous fermentation has hitherto received more attention than the acetous or putrid, not perhaps that it has any thing in it more remarkable, or more worthy of our conſideration, but becauſe it is in the natural order of things, to take more intereſt and ſet more value on what is of moſt immediate utility.

The vinous fermentation moſt attended to becauſe moſt uſeful.

The date of the diſcovery of the vinous fermentation appears too well eſtabliſhed to be called in queſtion. All hiſtorians agree in ſaying, that the moſt ancient nations knew how to prepare ſpirituous drinks. It aſcends therefore to the remoteſt times; and, if we pay any credit to the poets, we muſt carry it back to the fabulous ages. Indeed it would be ſurpriſing, if it eſcaped the notice of the earlieſt of men. An ebullition ariſing ſpontaneouſly in a liquid, a whole maſs riſing of itſelf, a ſweet liquor becoming vinous, the change of a ſaccharine matter into an ardent ſpirit, are all extraordinary things, calculated to ſtrike the attention, and awaken the deſire of tracing them to their firſt cauſes. Accordingly there is no phenomenon more early obſerved, and none that has been the ſubject of more conſideration, or has given birth to more experiments; yet, from one of thoſe contraſts that rarely occur in the annals of ſcience, though it has been the moſt ſtudied, there is not perhaps one, with which we are leſs acquainted. It has been a rock, on which the endeavours of chemiſts in all ages have ſplit. Becher, ſo celebrated for his ſubterranean phyſics, Stahl, the Neſtor of the ancient chemiſtry, Boerhaave, whoſe ideas were ſo great, Rouelle, to whom ſcience is indebted for part of the progreſs it has made during the laſt half century, and Macquer, that maſter in the art of writing, all failed in their attempts to penetrate this myſtery of nature. Lavoiſier, who was capable of ſurmounting the greateſt obſtacles, is the only perſon, who, enlightening the whole ſphere of chemiſtry by his genius, travelled this obſcure path without wandering out of his road. His inquiries into fermentation will ever remain a model of vegetable analyſis. In this, as in every thing elſe

Known to the moſt ancient nations.

Its phenomena excite attention,

and have led to many experiments;

hitherto little known.

All chemiſts have failed to explain it,

except Lavoiſier who alone made ſome progreſs in it.

His inveſtigation is a model of vegetable analyſis.

* Annals de Chimie, June 1803. p. 294.

he did, he obſerved that ſtrictneſs of deduction and accuracy of operation, which are his characteriſtics, and which may be conſidered as the ſource of the ſplendid diſcoveries, that will for ever illuſtrate his name. Still, notwithſtanding the excellence of what he did, he was far from leaving nothing more to be done. Though he ſhed great light on this part of ſcience, the obſcurity with which it was enveloped was ſo great, that it is ſtill ſeen only through a miſt. This is a truth that did not eſcape himſelf; he was well aware that he had only laid open the path to the goal, which no doubt he would have reached, no doubt he would have completed the career he had begun with ſuch ſucceſs, had not death, jealous of his fortune and glory, robbed ſcience of his labours.

But he left it imperfect.

In fermentation ſugar is converted into alcohol and carbonic acid, by means of an intermediate ſubſtance. But what is this? and how does it act?

All we know of fermentation in fact is confined to this, that the ſaccharine matter is converted into alcohol and carbonic acid by means of an intermediate ſubſtance. But what is the nature of this ſubſtance? and how does it act on the ſugar? Theſe two grand queſtions form the ſubject of the preſent memoir; queſtions that have been often attempted, without ever having been ſolved. Some have thought, that the fermenting principle reſided in the extractive matter. Others would have it to be in the mucilage, becauſe this more frequently accompanies the extractive matter. Some, again deceived by the preſence of tartar in wine, have imagined they found in this the true ferment; but if, inſtead of confining themſelves to the fermentation of the muſt of grapes, they had turned their attention to that of other juices, in which analyſis cannot diſcover the exiſtence of this ſalt, they certainly would not have fallen into this error. Others, laſtly, inconſiderately adopting all theſe opinions, have aſſerted, that a mixture of theſe different matters preſide as it were over fermentation, effecting the decompoſition of the ſugar, and its converſion into alcohol *.

Imagined to be extractive matter, mucilage, tartar, which laſt is not found in all fermenting liquors,

or a mixture of all theſe.

Of theſe hypotheſis ſome are evidently falſe; others are ſeducing, and acquire from ſpecious reaſoning ſome degree of probability. But before we admit them, we muſt conſult ex-

* Mr. Thenard here gives a long note, to prove he had not ſeen the prize eſſays written in 1787 and 1788 by Fabroni, from whom ſome perſons at Paris accuſed him of having borrowed, and to ſhow, that Fabroni and he differ eſſentially in this theory; but it is here omitted as irrelevant. T.

perience;

perience; we ought, leaving them all out of the queſtion, to deduce from obſervation the theory, which we are too apt to form beforehand. If the genius of Stahl, inſtead of giving birth to phlogiſton, a being that never exiſted but in the brilliant imagination of that extraordinary man, had interrogated nature by means of experiment more than he did, perhaps it would not have gone aſtray; perhaps Stahl would have diſcovered the truth, and deprived France of the glory of having produced the author of the modern theory of chemiſtry. Such is the courſe I have purſued. Before I formed or adopted any ſyſtem, I obſerved facts, and deduced from them conſequences, which, it appeared to me, muſt guide us to the view of what paſſes in liquors under fermentation. But in a ſubject of ſuch nicety nothing is more eaſy than to deceive ourſelves; and it is particularly for the purpoſe of correcting my notions, if they be not juſt, that I ſubmit to the claſs the reſult of my inquiries *.

Stahl miſled by hypotheſis and neglect of experiment.

Facts obſerved by the author.

My firſt obſervations were made on the juice of gooſeberries, which I had ſtrong reaſons to prefer to any other; its fermentation proceeding with moſt celerity, ſo that it is conſequently beſt calculated to throw light on the cauſes that produce it. All my reſearches were directed at firſt to diſcover the matter, that ſerves as a ferment. It would be making a great ſtep, and almoſt reſolving the problem, or at leaſt diſcovering a number of truths not yet known, to determine the nature of this matter, and to aſcertain whether it be always one and the ſame, or whether there be ſeveral that poſſeſs this property. This important queſtion had ſtruck me long ago: I had even meditated upon it occaſionally, and promiſed myſelf to attempt its ſolution, when the Inſtitute propoſed it as a ſubject of a prize. This was an additional ſtrong motive to my purſuing it. I was far from being diſpoſed to admit ſeveral fermentative principles; every thing led me to believe, that there was but one, and that it was none of thoſe hitherto ſuſpected, ſince in fact neither extractive matter, nor mucilage, nor tartar, &c. acts upon ſugar. But this required poſitive demonſtration; and though I have yet no abſolute proofs of it, as it is by no means demonſtrated, that there are ſeveral, and one is obſerved on all occaſions, this opinion ſeems to me at preſent to deſerve the preference.

He firſt examined gooſeberry juice, as fermenting moſt quickly;

in order to diſcover what ſerves as the ferment,

and whether it be one peculiar ſubſtance, or ſeveral.

He ſuppoſed it to be one and the ſame;

and this, though not demonſtrated, moſt probable.

* This paper was read to the National Inſtitute.

Gooſeberry juice contains a ſlightly glutinous matter, which, ſeparated by filtration,

occaſioned a mixture of ſugar and water to ferment,

and become vinous.

But as it ſcarcely effects this on ſix times its weight; it may be preſumed to contain but a ſmall portion of the fermentative principle.

This could not be obtained apart;

not apparently altered by having produced fermentation;

but it ceaſed to yield ammonia.

The germ of fermentation is therefore of an animal nature,

not ſeparable by reagents.

The author therefore had recourſe to fermentation.

Through a linen cloth of cloſe texture I preſſed out the juice of a killogramme of gooſeberries. It was turbid, and held in ſuſpenſion a ſlightly glutinous matter, which I ſeparated by the filter, and waſhed in a large quantity of water. As nothing is to be neglected in experimental ſcience, and the moſt trifling fact frequently leads to important conſequences, I ſubjected this matter to a regular examination. I firſt mixed it with ſugar and water, to ſee whether it would cauſe them to ferment; and I ſoon perceived many bubbles of an elaſtic fluid to be extricated, which I found to be carbonic acid. The efferveſcence continued a week, and at the end of this period the liquor was a pleaſant drink, but ſlightly ſaccharine; it contained a great deal of alcohol, and might eaſily have been miſtaken for a wine not yet completely made. It may be ſuppoſed I redoubled my zeal and attention in the examination of a ſubſtance, that offered me what I ſought. It was natural firſt to inquire, whether the whole of it were adapted to decompoſe ſugar; but a ſixth part of its weight being ſcarcely able to effect this decompoſition, I concluded, that it contained the fermentative principle only in ſmall quantity. This I attempted every method in vain to ſeparate, and obtain apart: nothing therefore remained for me, but to compare it before and after it had ſerved to produce fermentation. It was not apparently altered by this proceſs; being ſtill inſipid, inſoluble both in water and in alcohol, and affecting neither infuſion of litmus nor ſyrup of violets: but on diſtillation it no longer afforded any trace of volatile alkali. This reſult, at which I was not ſurpriſed, and which a ſecond experiment confirmed, was nevertheleſs a ray of light, that confirmed me in the courſe I ſhould purſue. It ſhowed me, that the germ of fermentation was of an animal nature, it agreed with the ideas I had before conceived, and gave to my ſuſpicions an appearance of reality.

I now examined the juice of gooſeberries with great care, to diſcover this animal matter, which I conſidered already as the true ferment. As it was inſoluble by itſelf, it muſt be combined with ſome ſubſtance, that held it in ſolution. All the reagents I employed failing to anſwer the purpoſe, I had recourſe to fermentation itſelf, and obſerved the phenomena produced by it under all circumſtances. I made my experiments on nearly a litre of filtered and perfectly clear juice.

The apparatus was placed in a ſtove, where the thermometer ſtood at 20°: it was not long before a fermentation was evident; a large quantity of carbonic acid was preſently evolved; much froth was formed; the liquor loſt its tranſparency, and it even became ſo turbid, that a ſediment was thrown down, which was more evident as the fermentation approached its end. This ſediment was of a yellowiſh white colour, glutinous, void of taſte, grew brown on drying in the open air, and became ſlightly acid. Thrown on redhot coals it burnt in the ſame manner as animal ſubſtances: diſtilled in a ſmall retort it afforded a conſiderable quantity of carbonate of ammonia even cryſtallized. It made ſugar ferment with extreme promptitude. In ſhort it was a ſubſtance perfectly analogous to the yeaſt of beer.

Filtered gooſeberry juice, in a heat of 20° ſoon fermented; evolved much carbonic acid, was covered with froth, became turbid, and depoſited a ſediment of a yellowiſh white, glutinous, inſipid, grew brown on drying, and ſlightly acid. On redhot coals burnt like an animal ſubſtance, and afforded carbonate of ammonia when diſtilled.

I was eager to try whether this phenomenon were general, as it ought to be according to my mode of reaſoning: and in fact experience ſoon taught me, that it was common to all juice in a ſtate of fermentation. The muſt of grapes, the juice of cherries, pears, peaches, aud apples, and the decoction of barley and of wheat, afford yeaſt in their fermentation. The grape juice yielded more than the others, though leſs than that of gooſeberries: accordingly it did not ferment ſo readily as the latter. The juices of cherries and peaches depoſited nearly the ſame quantity: thoſe of pears and apples afforded very little, which is the reaſon why their fermentation is ſo ſlow. I could have wiſhed to have had a greater number of fruits at my diſpoſal, that I might have varied my experiments more: they were ſufficient, however, to prove, that, where alcohol is formed, a ſediment of yeaſt is commonly formed likewiſe. If they who have any doubt on the ſubject remaining will maturely conſider the two following experiments, I believe they will find themſelves convinced. I knew that honey diluted with water would gradually be converted iuto a liquor containing ſpirits. Cullen informs us, that the ſaccharine urine of a diabetic patient undergoes in time the ſame change*. Accordingly I ſet both of theſe to ferment, and the ſediment of yeaſt was formed in each.

It ſoon fermented; and is perfectly analogous to yeaſt. This ſubſtance common to all fermenting juices.

Grape juice yields moſt next to that of gooſeberries.

The juices of pears and apples afford little, whence they ferment ſlowly.

Yeaſt commonly formed where alcohol is produced; even from honey and water, and diabetic urine.

* It was Dr. Dobſon of Liverpool, who firſt found, that diabetic urine changed firſt into a vinous liquor, and afterward into an acetous, before it became putrid. T.

It

So that in every ſpirituous fermentation an animal matter is depoſited ſimilar to yeaſt.

It may be laid down, therefore, as demonſtrated, that in every ſpirituous fermentation an animal matter is depoſited, ſimilar in all reſpects to that ariſing from wort, poſſeſſing abſolutely the ſame properties, and in particular that of decompoſing ſugar, and converting it into carbonic acid and ſpirit of wine. This gives riſe to a new queſtion, that naturally preſents itſelf, and ought next to occupy our attention. Is the yeaſt generated in the proceſs of fermentation, or rather was it already formed, and did ſerve as a ferment?

Is this generated in the proceſs of fermentation?

No direct proof, that nature employs it excluſively as a ferment.

I muſt confeſs we have yet no experiments, which directly prove, that nature employs this ſubſtance excluſively to effect the converſion of ſugar into alcohol and carbonic acid. For why ſhould it be depoſited when the fermentation has taken place? It may be ſaid indeed, that the ſugar holds it in ſolution, that it can diſſolve more than is neceſſary for its decompoſition, and that then the exceſs is precipitated. But this theory is feebly ſupported by experiment. Is this a ſufficient reaſon, however, to reject it altogether? Have we not ſeveral inſtances of compounds, that require much time for their formation? and this perhaps is what occurs in the juices of fruits, where the ferment and the ſugar are long in contact with each other. What is certain, or what at leaſt appears probable, is, that, if yeaſt be a product of fermentation, as all liquors that ferment depoſit it, no doubt it owes its origin to one and the ſame ſoluble ſubſtance, from which probably it differs little, and which produces it by its reaction on the ſugar.

If it be a product of fermentation, it probably comes from ſome particular ſubſtance, little changed.

Yeaſt an immediate principle of vegetables.

Whichever of theſe two opinions obtains the preference, after more numerous trials, as I have no doubt, that yeaſt is an immediate principle of vegetables, and in conſequence acts an important part in the phenomena both of art and of nature; as I am likewiſe perſuaded, that, if any other ſubſtance capable of exciting fermentation exiſt, it is in the higheſt degree analogous to yeaſt; that it differs from it very little; that it is compoſed like it of azote, oxigen, carbone, and hidrogen; and laſtly that it has no doubt the ſame mode of acting on ſugar: I ſhall proceed to exhibit with the greateſt care the properties of this matter, which I ſhall henceforward term ferment, and in particular conſider well its action on the ſaccharine principle, in order to eſtabliſh the theory of fermentation. This theory, even ſuppoſing yeaſt not to exiſt already formed, but to be produced in fermenting juices, will ſtill be of utility, and capable of various applications, as will appear below.

and if there be any other ferment, it muſt be ſimilar to it, and compoſed of azote, oxigen, carbone, and hidrogen.

I ſhall

I ſhall not recapitulate the phyſical qualities of ferment, as I have mentioned them ſeveral times already; but I ſhall confine myſelf to its chemical properties, which alone are of eſſential import. It has no taſte. It neither reddens infuſion of litmus, nor changes ſyrup of violets green. The putrid fermentation, which in time it undergoes, is in every reſpect ſimilar to that of animal ſubſtances. By deſiccation it loſes three fourths of its weight, and this loſs conſiſts entirely of water. Thus dried it is ſtill capable of exciting fermentation; it is by no means decompoſed, and may be preſerved in this ſtate without alteration for an indefinite time. We may avail ourſelves of this property, to convey it to places at a diſtance from any brewery, or with which it is ſo difficult to keep up an intercourſe, that freſh yeaſt could not be ſent to them, particularly in ſummer, without becoming putrid. Diſtilled in a ſmall retort, and urging the fire ſo far as this would bear, eight parts of ferment left a reſiduum of 2.83 of coal; and I obtained 1.61 of water, 1.31 of oil, and 1.46 of muriate of ammonia on adding muriatic acid. Finally, I collected 0.33 of gas, containing a fifth of its bulk of carbonic acid, and which, when this was ſeparated from it by potaſh, burnt like carbonated hidrogen, and required 1.5 of oxigen for its combuſtion. From this experiment we ſee, that ferment contains in particular a great quantity of carbon.

Chemical properties of *ferment*.

May be preſerved any length of time when dried.

Analyſis.

Water at the temperature of 12° or 15° does not diſſolve $\frac{1}{400}$ of ferment: indeed it diſſolves ſo little, that after ſtanding upon it ſeveral hours, and being well filtered, it has ſcarcely any action upon ſugar. Boiling water occaſions it to undergo a decompoſition, which I ſhall examine in another memoir.

Inſoluble in water.

Boiling water decompoſes it.

Nitric acid, even diluted with water, at eighteen degrees, decompoſes it alſo: it converts it into greaſe; and there is evolved from it at firſt azot mingled with carbonic acid, and afterward nitrous gas at the ſame time.

Nitric acid converts it into fatty matter; and azot with carbonic acid, and nitrous gas are given out.

Potaſh acts upon this ſubſtance in the ſame manner as upon animal matters, and the phenomena in both caſes are perfectly the ſame: with each it forms a ſaponaceous ſubſtance, and a great quantity of ammonia, that is volatilized.

With potaſh it forms ſoap, and ammonia is extricated.

But of all the properties of ferment, no one is ſo remarkable, and at the ſame time ſo uſeful, conſequently no one ſo much deſerves to be ſtudied, as its action upon ſugar: this is intereſting to men in every claſs of ſociety, from the mechanic to the

Its action upon ſugar.

the philoſopher; to both by its products, and to the latter becauſe it may be a fertile ſource of reflection and of new truths. It is much to be regretted, that Lavoiſier did not purſue the inveſtigation as he intended, and examine it with that care, which is conſpicuous in all his labours. Who was more capable of giving birth to a theory of fermentation, than the author of the modern theory of chemiſtry? No doubt he was prevented from doing this by a concurrence of circumſtances: and this theory, important as it is to ſcience, has hitherto remained vague and hypothetical. Knowing the fermentative principle, it could not avoid naturally making a part of my reſearches: if I have not rendered it as clear as I hoped, at leaſt the veil with which it was covered is removed, and it reſts on reaſoning confirmed by experiment.

To obtain the ſolution of this problem, I added together different quantities of ferment and ſugar; I obſerved in every caſe what became of both; and I confirmed by farther obſervations what the preceding had ſuggeſted. Sixty grammes (927 grains or nearly one ounce of ferment) not dried, and three hundred grammes (4630 grains) of ſugar entered into fermentation readily, the temperature being 15° (centigrade=59° Farenheit.) In four or five days all the ſaccharine matter had diſappeared; 51.5 litres (3041 cubic inches) of carbonic acid had been evolved; the liquor being filtered, and diſtilled to two thirds, gave on a ſecond rectification 863 grammes of ſpirit at 13°. The apparatus was ſo contrived, that nothing was loſt: the receivers were cooled with common ſalt and ice. I found by ſynthefis, that this quantity of ſpirit was equivalent to 171.5 grammes of alcohol at 39°. The reſidues left after diſtilling the ſpirit were poured into diſhes and evaporated to dryneſs: from the reſiduum of the ſecond diſtillation nothing was obtained, but that of the firſt yielded about 12 grammes of a nauſeous ſubſtance, ſlightly acid, and feebly attracting the moiſture of the air. I wiſhed to diſcover the nature of this acid, but there was too little to aſcertain it. Lavoiſier ſays, it is the acetous. Laſtly, of the ſixty grammes of ferment, there remained forty grammes of a ſubſtance, which I believed to be more animalized than the ferment itſelf. I was much ſurpriſed to find, that I obtained from it by diſtillation much leſs ammonia. Hence I ſuſpected, that by mixing it afreſh with ſugar, fermentation would again take place, and thus

Of the precipitate one part with five of ſugar fermented readily.

Products of carbonic acid and ſpirit obtained,

equal to 171.5 of alcohol.

The reſiduum 12 grammes, nauſeous and, ſlightly acid.

Lavoiſier ſays the acid is the acetous.

Reſidue of the ferment.

thus all the azot would diſappear. What I had foreſeen occurred: at the end of ſeven days, having filtered the liquor, I obtained as a reſiduum thirty grammes of a ſubſtance, which on diſtillation gave no trace of volatile alkali. I was perſuaded, that the azot was carried off with the carbonic acid gas. To convince myſelf of this, I collected near 41 litres of the carbonic acid in an inverted veſſel filled with ſolution of cauſtic potaſh. The whole was abſorbed, which leaves no doubt of its purity.

Produced fermentation afreſh, and thus loſt all its ammonia.

This was not carried off with the carbonic acid.

What then becomes of the azot? it ought to be found either in the reſiduum of the ferment, or in the reſiduum obtained by evaporating the liquor left after diſtillation, or in the alcohol: but the reſiduum conſtitutes only half the ferment employed; the quantity of matter left by 300 grammes of ſugar and ſixty of ferment amounts only to twelve grammes; and neither of theſe yields any ammonia on diſtillation, while ferment affords a great deal. If theſe obſervations be juſt, if I have accurately noticed all the phenomena, if nothing have miſled me, we cannot avoid concluding, that the azot muſt exiſt in the alcohol. Yet I have ſought in vain to diſcover its preſence in this fluid, in ether, and in the acetous acid: on paſſing theſe through tubes heated red-hot in the fire, and burning the gaſſes in Volta's eudiometer by means of the electric ſpark, we obtain ſuch ſmall quantities, that they are by no means ſufficient to decide the queſtion: 24 or 25 parts of gas yield at moſt one of reſiduum.

What became of the azot?

It muſt exiſt in the alcohol, though not diſcoverable there.

I have made ſeveral other experiments however, which hitherto tend to ſhow, that azot may exiſt in ſuch a manner as not to be diſcovered on diſtillation; conſequently that it may be one of the conſtituent principles of vegetables, though in general when diſtilled they afford no ammonia. But as I have not repeated theſe experiments, and they are of ſuch importance, that we cannot be too reſerved in announcing them, I purpoſe to reviſe and vary them, I will endeavour to appreciate all the circumſtances, and if I obtain convincing proofs, I will not delay communicating them to the claſs.

Azot may exiſt ſo as not to be detected by diſtillation, and therefore may be a conſtituent principle of vegetables.

However this may turn out, theſe reſults afford us ſufficient light, to ſee what paſſes in the act of fermentation. In this reſpect I cannot agree in opinion with Lavoiſier. I do not believe as he does, that all the carbonic acid formed proceeds from the ſugar. How, on ſuch a ſuppoſition, can we conceive the

All the carbonic acid formed does not proceed from the ſugar.

the ferment to act upon it? I think, that the firſt portions of acid are owing to a combination of the carbon, of the ferment, and the oxigen of the ſugar, and that the ferment gives riſe to fermentation by abſtracting from the ſugar a portion of this principle. To render this idea more clear, I ſuppoſe a particle of ſugar to be formed of eight parts of oxigen, four of carbon, and one of hidrogen, which is not very remote from the truth, according to the experiments of Lavoiſier: one of theſe eight parts of oxigen will unite with a fourth of a part of the carbon of ferment, and then, the equilibrium between the principles of the ſugar being diſturbed, they will combine in a different manner, ſo as to form carbonic acid and alcohol.

Ferment produces fermentation by taking from the ſugar a portion of its oxigen.

Ferment has a ſtrong attraction for oxigen, as is proved by its decompoſing air.

The ferment has in fact a ſtrong attraction for oxigen; as is proved by its decompoſing air with the greateſt facility; when acetous and carbonic acids are produced, and the azot is diſengaged. If pure air be employed inſtead of common air, the reaction is ſtill more ſpeedy. I have introduced 15 grammes of ferment into a veſſel filled with a litre of pure air; I opened it over quickſilver; a fifth of its bulk was abſorbed; the ferment was grown ſour, all the oxigen gas had evidently diſappeared, and was converted into carbonic acid: the temperature was 15°.

What becomes of the principles of ferment.

From what has been ſaid we ſee what becomes of the carbon of the ferment, and we ſhall learn what may become of its other principles, if we recollect the quantity of products reſulting from a given quantity of matter ſubjected to fermentation, and comparing the nature of the one with the other.

From ſixty grammes of ferment, and three hundred grammes of ſugar we obtained, carbonic acid 95 grammes; pure alcohol 171.5; extractive matter, ſlightly acid, and containing no azot, 12; reſiduum of the ferment 40. Theſe 40 grammes ſtill contained 25 of ferment, ſo that 35 only had been employed for the decompoſition of the ſugar; and theſe 35 were reduced to 15 of a white ſubſtance* inſoluble in water, incapable

Leaves a peculiar ſubſtance.

Yeaſt is ſeldom pure, containing more or leſs of this peculiar ſubſtance, with which it forms the chief part of wine-lees.

* The yeaſt depoſited by a fermenting juice is ſeldom pure: in general it contains more or leſs of this peculiar white ſubſtance, which is ſometimes ſo abundant, that the ſediment conſiſts of it almoſt entirely. It is the mixture of theſe two ſubſtances, that conſtitutes in great part the lees of different wines. All theſe effects depend on the relative proportions of the ferment and the ſugar; if there be little ſugar and much ferment, the yeaſt will be pure; if on

incapable of acting upon ſugar, yielding no ammonia on diſtillation, and leaving a coal, that burns with ſcarcely any reſiduum; in ſhort, exhibiting characters that diſtinguiſh it from all other ſubſtances, and induce me to conſider it as a peculiar matter.

It appears then, that ferment takes oxigen from ſugar, not only by means of part of its carbon, but alſo by means of part of its hidrogen; for the quantity of carbon given out by ferment is too ſmall, to be the ſole germe of fermentation. The azot diſappears, and enters perhaps into the compoſition of the alcohol; the other principles of the ferment form acetous acid, and a peculiar inſoluble white matter, which is precipitated. The acetous acid remains in ſolution in the liquor left after diſtillation, with an extractive matter, proceeding no doubt from the ſugar, and foreign to it.

The hidrogen of the ferment acts as well as its oxigen in producing fermentation.

It is not probable, that the elements of the ſugar, in their reaction upon each other when the equilibrium between them is diſturbed, form water: there is very little hidrogen in ſugar, and a great deal in alcohol; and beſides, on adding together the quentity of carbonic acid, of alcohol, of extractive matter, and of reſiduum, we have a deficiency of one eleventh only of the matter by which they were produced. This loſs muſt be attributed to the water the ſugar contains, and is by no means owing to alcohol's being carried off by the carbonic acid. Of this I have been convinced myſelf by receiving more than thirty litres of this gas in cauſtic potaſh; and by diſtillation and rectification I obtained only a few grammes of fluid, which had ſo little taſte of ſpirit, that it was not to be diſtinguiſhed.*

The elements of the ſugar do not form water.

A loſs of one eleventh owing to water contained in the ſugar. No alcohol carried off by the carbonic acid.

This

on the contrary there be much ſugar and little ferment, the ſediment formed will contain but little yeaſt, and perhaps even none; and then it will conſiſt entirely of the white ſubſtance here mentioned, while the ſupernatant wine will or may be of a ſaccharine quality, Thus ſhould yeaſt even be a product of fermentation, it would act an important part in this phenomenon; and if it do not give riſe to it in the firſt place, which is not certain even in this caſe, at leaſt it continues to give riſe to it. This was the reaion why I ſaid before, that in all caſes nature afforded us numerous applications of the theory I laid down.

This depends on the proportion of the ferment to the ſugar.

* Cit. Seguin has laid before the Inſtitute a very different theory of fermentation. He thinks, that in this proceſs water is decom-

Seguin's theory of fermentation.

poſed,

This theory agrees with the facts, though not yet complete.

This theory, appears to me ſo much the more probable, becauſe it perfectly accords with facts; and this truth becomes particularly ſtriking when we compare them. I am far, however, from conſidering it as complete. Time no doubt will beſtow on it the perfection it wants; and I hope myſelf ſoon to add to its evidence. I know not whether I ſhall be able to diſcover what becomes of the azot of the ferment: but I ſhall aſcertain without difficulty, whether the reſidual matter obtained, which I conſider as a peculiar ſubſtance, be a product of fermentation, as I believe; whether the ſugar contribute to its formation, which is poſſible; or whether it be ready formed, and merely precipitated, which is contrary to all probability. The experiments, that remove all my doubts on this head, I ſhall relate in a ſecond memoir, in which I ſhall not only elucidate ſuch points in this as may appear equivocal, or at beſt reſting on too ſlight foundations, but I ſhall alſo exhibit all the particulars that reſult from them. Here on the contrary I have avoided them as much as poſſible, and endeavoured to conſider the phenomenon only in a general way.

What remains to be done.

VII.

Conjectures reſpecting the Formation of the Ice in the Cavern of Grace-Dieu. By C. L. Cadet, *of the College of Pharmacy**.

A cavern 146 feet underground, its entrance 60 feet broad and 80 high, its breadth 135 feet.

ABOUT ſix or ſeven leagues from Beſançon, near the village of Beaume, and half a league from the abbey of Grace-Dieu, there is a natural grotto, 146 feet below the ſurface of

poſed, that its oxigen unites with the carbon of the ferment, and produces carbonic acid, while its hidrogen combines with the ſugar, and converts it into alcohol. For this theory to be admiſſible, we ſhould obtain more alcohol than there was ſugar; but the fact is, little more than half its weight is produced, and beſides, ſuppoſing all the carbon of the decompoſed ferment to be converted into carbonic acid, at moſt not above a ſixth part of the quantity actually produced would be formed, as we may eaſily convince ourſelves from calculations already eſtabliſhed. It may likewiſe be objected to this theory, that ſugar contains a great deal of oxigen, and alcohol very little.

* *Annales de Chimie*, No. 134, Feb. 1803, p. 160.

the

the plain. The entrance of it is 60 feet wide, and about 80 high. Within its greateſt breadth is 135 feet. The ſtone that forms the rock is calcareous carbonate, in part lamillated.—This grotto is diſtinguiſhed from all others by a very ſingular phenomenon: in ſummer ice is conſtantly formed in it in large quantities, and this ice diminiſhes at the approach of winter.

Ice is formed it in ſummer, in and diminiſhes in winter.

As this natural ice-houſe has been accurately deſcribed by Mr. de Croiſmare in the Encyclopédie, and ſeveral other deſcriptions agreeing with his are to be found in the Memoirs of the Academy of Sciences for 1712, thoſe of the *Savans Etrangers* for 1743, the works of Mr. le Cat and Ravier, and Madame Ganthier's Tour of a French Lady in Switzerland, I ſhall confine myſelf to a few obſervations, that have eſcaped the notice of many of the learned who have viſited it, and combine to explain the formation of the ice in this cavern.

Deſcribed by various writers.

I viſited it about the end of September, 1791, and then found but little ice. My guide, who was accuſtomed to conduct travellers to it, informed me, that a month before the ice was three times the bulk of what I ſaw. The perſon who ſells refreſhments at Chammars, a public garden at Beſançon, told me the ſame. He ſaid, that when the winter had proved mild, ſo that they were not able to fill the ice-houſes in the city, he ſent in ſearch of ice to the cavern of Grace-Dieu, and that he choſe for this the warmeſt days, *becauſe then there was moſt ice* in the cavern. Meſſrs. de Croiſmare, le Cat, and Ravier, in their accounts, equally atteſt, that in ſummer the ice is more abundant. The inhabitants whom I met with on the mountain, and of whom I enquired concerning this fact, all told the ſame thing. Conſequently, though the variation of the ice is queſtioned by Mr. Dunod, quoted in the work of Madame Ganthier, I readily admit the exiſtence of this phenomenon, particularly as I think I can explain it.

In September, 1791, it had but little ice.

A month before it had thrice the quantity.

Ice there in mild winters.

By ſeveral authorities the ice is moſt plentiful in ſummer.

The rock that forms the roof of this grotto is lower than all the neighbouring mountains, and even than the ſurface of the plains. Its temperature therefore ought to be mild, like that of all ſubterraneous caverns, except at the moment when ice is accumulated there in abundance. Ten waggons would have been ſufficient to carry away all I ſaw, and the air of the grotto did not feel colder to me than the open air. The authors I have quoted ſay, that the cold in it is below 0. I had no thermometer, but I can affirm that this aſſertion is at leaſt queſtionable.

The rock that forms the roof low.

The air in it does not feel cold.

Deſcription of the cavern.

A juſt idea of the ſituation of the cavern is ſufficient to ſhew in what manner the water percolates the ſtrata of rock, and falls perpetually from the roof. The cavern is ſurrounded by thickly wooded mountains, and from one of the neareſt rocks ruſhes a torrent, that forms three caſcades, and turns ſeveral mills. But what is remarkable is, that the water filtering into the cavern does not freeze as it falls: it is received into a baſin of two or three feet diameter formed in the ice, and remains fluid on a level with the ſides of the baſin. I drank ſome of this water, which did not ſeem to me very cold. It was perfectly clear and good.

Clear water burſts through the rocks and falls into a baſon of ice.

The grotto is covered by buſhy trees.

I obſerved, that the grotto is not only ſurrounded by wood on all ſides, but that it is covered by a number of the moſt beautiful beech, and buſhy cheſnut trees. Theſe deny all acceſs to the rays of the ſun, and always preſerve a cool damp air round the cavern. This ſhade however would not be ſufficient to produce an icy air, but the leaves of theſe trees are ſo many organs of perſpiration, that pour into the atmoſphere the moiſture which exhales from their veſſels, and that has been pumped up by their roots. This evaporating water cannot paſs from the ſtate of a fluid to that of a gas, without abſorbing a large quantity of caloric: the ground therefore is continually cooled by the evaporation the trees produce. In winter the warmth of the ſun no longer draws up moiſture; the leaves, which botaniſts conſider as real lungs, have diſcontinued their organic action; and the ſame evaporation no longer exiſts. The temperature of the cavern then changes, and approaches that of ſubterranean places and grottos in general. Several facts ſerve to confirm this theory, and to prove, that the cold produced in the cavern of Grace-Dieu is entirely owing to the ſpeedy evaporation of the ſurrounding moiſture.

The ground is cooled by the evaporation in ſummer,

which does not take place in winter.

The trees over the entrance being cut down, leſs ice was formed.

Mr. Ravier, ſecretary to the biſhop of Bellay, ſays, that in 1724, the entrance of the grotto was overſhadowed by large buſhy trees, that theſe were cut down, and that the quantity of ice afterwards formed has been leſs.

The entrance was partly cloſed up by a wall,

He alſo reports, that Mr. de Vanolles, intendant of the province, deſirous of preſerving this natural curioſity, cloſed up the grotto by a wall twenty feet high, with a ſmall door, the key of which was entruſted to the bailiffs of the village, with a charge, that, no one ſhould be ſuffered to enter for the purpoſe of carrying away the ice. But this precaution produced

by which the ice was diminiſhed.

an unexpected effect: the ice perceptibly diminiſhed, and they were obliged to pull down the wall, which was in ſome meaſure an obſtacle to the evaporation. I ſaw the remains of this wall, and the inhabitants of Grace Dieu confirmed to me the fact mentioned by Mr. Ravier.

The formation of the ice in this curious grotto, therefore, may be explained, I believe, according to the theory of the jars that ſerve to cool the water they contain in hot countries, by permitting part of the fluid to tranſude through their porous ſides.

This ice ſtated to be formed in the ſame way as water in porous jars is cooled in hot countries by the evaporation from the outſide.

As no natural philoſopher has yet attempted to explain this intereſting phenomenon, I have flattered myſelf that I might be indulged in a conjecture, founded on phyſical principles fully eſtabliſhed by experience.

VIII.

Obſervations on the Structure of the Tongue: illuſtrated by Caſes in which a Portion of that Organ has been removed by Ligature. *By* EVERARD HOME, *Eſq. F. R. S.**

(Concluded from VOL. VI. *Page* 379.*)*

ENCOURAGED by the reſult of this caſe, I was led to perform a ſimilar operation upon a perſon at a more advanced period of life.

Another caſe treated in the ſame manner,

Margaret Dalton, 40 years of age, was admitted into St. George's hoſpital, on the 25th of December, 1801, on account of a tumour, the ſize of a pea, ſituated on the right ſide of the tongue, near its edge. The hiſtory of the caſe was as follows. A ſmall pimple appeared, and gradually increaſed, without pain; the only inconvenience was, that it affected her ſpeech, and, when bruiſed by the teeth, bled freely.

The operation was performed on the 11th of January, 1802, in exactly the ſame manner as has been already deſcribed. It produced a conſiderable degree of ſalivation, which was extremely troubleſome, (much more ſo than the pain the ligatures produced,) and continued till the ſlough came away. The ligature neareſt the root of the tongue ſeparated on the 6th day; the other on the 7th; and, in three days after the ſeparation of the ſecond ligature, the wound was completely ſkinned over.

with complete ſucceſs.

A third caſe.

A third caſe of this kind came under my obſervation, in which there was a ſmall tumour in the ſubſtance of the tongue, about the ſize of a pea, which gave me the idea of its being of that kind which might terminate in cancer. The patient was a gentleman of about 41 years of age. Upon examining the tumour, I told him of my alarm reſpecting its nature; and at the ſame time added, that I was very ready to remove it, ſhould it be the opinion of other practitioners that ſuch a ſtep was adviſeable; and my experience in two former caſes led me to believe it might be done with ſafety. I therefore adviſed him to conſult other medical practitioners of reputation, and acquaint me with their opinion. Mr. Cline was conſulted, and his opinion coincided with mine; which made the patient decide upon having the tumour removed.

The operation, and hiſtory of its progreſs and ſucceſsful termination.

The operation was performed on the 28th of December, 1802. The needle pierced the tongue an inch beyond the tip, a little to the right of the middle line of the tongue; and the ſpace between the two ligatures, when they were tied at the circumference of the tongue, was fully an inch. The tongue was thick; and the maſs included by the ligatures was ſuch as to make it difficult to compreſs it. The operation gave conſiderable pain, of a numbing kind. Immediately after the operation, the part included became dark coloured, particularly towards the middle line of the tongue. A ſalivation took place. The next day, the pain and ſalivation were great, and the patient could not ſwallow; but, on the day following, he could take broth, negus, and other fluids.

On the 6th day from the operation, the ſlough became looſe; and the leaſt motion of the tongue gave great pain. Upon examining the ſlough, there was a ſmall ſpot which looked red, and was ſurrounded by a dark ſurface; this was towards the right ſide. Upon further examination it appeared, that the ligature to the right had not completely deadened the part at the centre, in which the artery had its courſe. This accounted for the red ſpot, as well as for the pain the patient ſuffered; and led me, on the ſeventh day, to diſengage the ligature on the left, (which was almoſt completely ſeparated,) by means of a pair of ſciſſars, and paſs another ligature through the grove to the oppoſite ſide, and tie it over the part not completely deadened. This gave great pain for a few hours, which was relieved by the uſe of tincture of opium.

opium. On the 8th day, the patient had leſs pain than on any preceding day, and leſs ſalivation; and, on the 9th, the whole ſlough came away. On the 13th, the tongue had ſo much recovered itſelf, that there did not appear any loſs of ſubſtance whatever, only a fiſſure of half an inch in depth, in the anterior part of it; and, as that now ſeemed to be exactly in the centre, there was not the ſmalleſt deformity.

General reſults of the preceding caſes or experiments.

The preceding caſes, in the view which it is intended to take in the preſent Paper, are to be conſidered as ſo many experiments, by which the ſtructure of the tongue is in ſome reſpects aſcertained: they enable us to draw the following concluſions.

Internal ſtructure of the tongue has little irritability.

The internal ſtructure of the tongue is leſs irritable than almoſt any other organized part of the body; therefore, the peculiar ſubſtance which is interpoſed between the faſciculi of its muſcular fibres, is not in any reſpect connected with the nerves which paſs through its ſubſtance to the organ of taſte, but is merely a ſoft medium, to admit of great facility of action in its different parts.

its nerves eaſily deadened.

The nerves of the tongue appear to be more readily compreſſed, and deprived of their power of communicating ſenſation, than nerves in general; and any injury done to them is not productive of diſeaſed action in the trunk of the injured nerve.

Comparatively little inflammation and pain follow compreſſion of this organ.

If we compare the effects of compreſſion upon a portion of the tongue, with thoſe of a ſimilar compreſſion upon the hæmorrhoidal veins when they form piles, or thoſe of the teſticle in caſes of varicoſe veins of the ſpermatic chord, which not only produce very violent local inflammation, but alſo a conſiderable degree of ſymptomatic fever, it is impoſſible not to be ſurpriſed that the reſults ſhould be ſo very different; ſince we are led to believe, upon a general principle, that parts are ſenſible in proportion to their vaſcularity, and that all the organs of ſenſe, when inflamed, are more exquiſitely ſo than any other parts of the body.

It ſloughs off very early.

The tongue appears to have a power of throwing off its ſloughs in a ſhorter time than any other part. Eight or nine days is the ordinary time of a ſlough ſeparating from the common parts: in the boy's tongue, it was only five.

Having ſtated the information we derive from theſe caſes, reſpecting the ſtructure, ſenſibility, and irritability of the

tongue, it now remains to mention the advantage to be derived from them in a profeſſional view; and, although this is not directly in the line of the purſuits of this learned Society, yet, ſo ſtrongly is it connected with humanity, that it cannot be ſaid to be foreign to them, or undeſerving their attention.

Great benefit of this knowledge in caſes of cancer.

The information derived from theſe caſes, enables us to attempt with ſafety, the removal of any part of the tongue which may have taken on a diſpoſition to become cancerous. As this diſeaſe in the tongue always begins in a very ſmall portion of that organ, it is, in the early ſtage, more within the reach of removal than when in any other part of the body; and, as the glands of the tongue are independent of each other, the cancerous diſpoſition by which one of them is attacked, does not ſo readily communicate itſelf to the others; and the part may be removed, with a greater degree of ſecurity againſt a future recurrence of the diſeaſe, than in other caſes where this malady attacks a portion of a large gland, the whole of which may be under the influence of the poiſon, long before there is any appearance of its being diſeaſed.

IX.

Account of Three Mechanical Structures not commonly noticed or known, in which the Effect is produced by the Difference between the Actions of Two ſimple Inſtruments of the ſame Deſcription; namely, 1. a compound Barrel and Winch; 2. a compound Screw, and 3. a compound Pump. W. N.

Properties of the wheel and axle.

IN the ſimple mechanical engine, called the wheel and axle, or barrel and winch, the advantage of the power over the weight is known to be as the radius of the wheel or winch is to that of the axle or barrel. If it were, therefore, propoſed to increaſe that advantage, the handle muſt either be made longer or the barrel more ſlender; neither of which can practically be done beyond certain limits in the uſual form; becauſe the handle might be too long for management, or the barrel too ſlight to ſupport its burthen. The celebrated George Eckhardt is, I believe, the inventor of the conſtruction, *Plate* III. *Fig.* 2. in which the part A of the barrel is larger than the

A compound barrel or roller, by which the purchaſe may be greatly augmented.

part

part B, and the rope which paſſes under the pulley C and ſuſtains the weight D is wound upon each in contrary directions. Whenever, therefore, the handle is turned, ſo as to gather the rope upon the larger cylinder, it will be given off by the ſmaller: and, for every turn of the larger or its correſpondent portion of rope wound up, there will be given off a portion of rope, anſwering to the circumference of the ſmaller. Conſequently, the quantity of unwound rope will be leſs, after ſuch a turn, by a quantity equal to the difference between the circumferences of the two cylinders; and the weight D will be elevated through half that ſpace. Whence

Its power.

As the circumference deſcribed by the winch (or its radius)
Is to half the difference between the circumferences of the cylinders (or half the difference of their radii)
So is the power
To the weight (in equilibrio.)

Peculiarity of this engine.

It is evident that the peculiarity of this engine is that the half difference of the radii may be made as ſmall as we pleaſe without weakening the cylinder or requiring any quick curvature of the rope; neither of which requiſites could be had in the ſimple cylinder. Its practical diſadvantage appears to be that a large quantity of rope will be uſed to produce a moderate elevation of the weight.

Double ſcrew engine. One ſcrew preſſes down while another draws up, and the effect is produced by the difference of their actions.

The firſt account I find of the double ſcrew *Plate* IV. *Fig.* 1. is given by Mr. Wm. Hunter in the Philoſophical Tranſactions for 1781. In the ſketch before us, A B C repreſents the ſcrew of a preſs: the upper part of the ſcrew A has a coarſer thread than the lower B and a concave ſcrew or nut, C, receives the latter, while the former paſſes through a proper helical cavity in the frame of the preſs. In the preſent ſtate of the apparatus, the nut C is pinned faſt to its ſcrew by a croſs bolt; and conſequently, when the whole ſcrew is made to turn by the arms at the top, the nut C is carried round, and the effect is preciſely the ſame as if the preſs were governed by the ſcrew A alone. But when the nut C is diſengaged by drawing the croſs bolt, and likewiſe prevented from revolving by pinning it to the moveable platform, the effect becomes more compounded: that is to ſay, while the whole ſcrew is carried downwards by one revolution, through one thread of A, the nut C is drawn upwards through one thread of C. And-

the conſequence is the ſame as if a ſingle ſcrew had been uſed, having the diſtance between thread and thread equal to the difference beween the meaſures of one thread in each.

Examples of ſlow actions with ſtrong coarſe ſcrews.

For example. Suppoſe it were required to operate by ſtrong preſſure with a ſcrew of ten threads in the inch, it is evident that the metallic protuberance or helix could not be quite ſo thick as one twentieth of an inch, and could not therefore withſtand any conſiderable force. But by the preſent conſtruction we may make the thread as ſtrong as we pleaſe. Thus if the ſlighteſt be one quarter of an inch, the fineſt ſcrew muſt have two turns in the inch, and the coarſeſt two turns in one inch and one tenth; or in other words, it would paſs over the ſame ſpace in ten turns as the other ſcrew would in eleven.

It is evident that by the conſtruction in the figure, the moſt powerful preſſure need not be uſed till the ſubject under compreſſion has already been urged by the ſimple ſcrew.

Obſervation on new or unuſual conſtructions.

I may here remark concerning every one of the contrivances in this paper, that I have never conſidered it as a requiſite in offering a machine or inſtrument to the attention of my readers that it ſhould be univerſally better than the means already in uſe. If it be new or unheeded, and ingenious, or capable of adding to the ſtock of reſources which intelligent conſtructors find ſo various in their degrees of eligibility under different circumſtances, it will, I am confident, be conſidered with intereſt, and accepted with welcome.

Conſiderations reſpecting the uſe of a double ſcrew in meaſuring.

When we conſider the extreme preciſion of a ſimple ſcrew, if commonly well made, it may ſeem ſcarcely neceſſary to uſe this contrivance in the meaſurement of ſmall quantities. The fineſt ſcrews uſually to be met with do not exceed 100 threads in the inch, and I do not know of any having been made finer than of 200 threads in that ſpace. Let us conſider by what threads of uſual or even coarſe number we could have a difference per turn of above one thouſandth of an inch, and we ſhall find that the difference between a thread of 32 and one of 33 in the inch is the 1056 part of an inch. So likewiſe the difference between the 60th and 61ſt of an inch is the 3660th part, &c. I have a level trier on this conſtruction which gives the four hundredth of an inch for each turn of its compound ſcrew.

Pump with a double plunger by which the

Fig. II. *Plate* 4. is a pump deſcribed to me by Mr. Bramah, and I think his own invention. Between the collar of leather

A, there paſſes a ſolid cylinder, of which the lower part B (diminiſhed in diameter) paſſes out through another collar of leathers D; the internal ſpace or pump barrel ſituate between thoſe two collars being cloſed on all ſides, except where two pipes communicate, one of which, E, has a valve opening inwards, and the other D, has a valve opening outwards. Now when the ſolid cylinder or plunger is drawn up, the larger portion paſſes for the moſt part out of the cavity of the barrel; but inſtead of being replaced by an equal maſs of water entering through the pipe E as uſual in the jack head pump, it is moſtly replaced by the introduction of the ſolid part B, and the water which enters will be equal in its ſolid contents to the difference only between the ſolidities of the two cylindrical parts of the ſame length. Or the effect will be the ſame as if the barrel were of much ſmaller dimenſions. In this way with the ſtrength of conſtruction and ample fittings of a large engine the power of a ſmall one may be obtained, which in ſome inſtances of forcing may be deſirable.

ſpace for water in a large bore is reduced to that of a much ſmaller.

It ſcarcely need be mentioned that the ſame effect may be produced by two ſolid piſtons working in diſtinct barrels of different bore or ſtroke, and communicating with each other.

X.

Scoteography, or the Art of Writing in the Dark. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

PERHAPS you have remarked that my communications to your Journal are not all in the ſame hand: the truth is, I am unable to write; and my ignorance of the art ariſes from the loſs of ſight in early infancy. The knowledge of the preceding circumſtance cannot prove of the leaſt moment to you; it is only mentioned by way of introduction to the preſent letter, which may afford an uſeful hint to perſons labouring under the ſame diſadvantage.

Hiſtory of the invention.

The peruſal of Mr. Berard's method* of teaching the blind

* Phil. Jour. Nov. 1802, Vol. III. page 190.

to

to write was perhaps the firſt occurrence of my life which led me to think ſeriouſly of committing my thoughts to paper with my own hand.

Berard's method of writing by the touch only is not adapted to the early blind.

This gentleman's plan however preſented many difficulties which it is unneceſſary to enumerate; in reality it promiſes to be of much advantage to thoſe who have learned to manage a pen before they loſt the uſe of their eyes: but as this has not been my lot, I reſolved to conquer the hardſhips of fortune, if poſſible, by inventing an alphabet conſiſting of characters which a mechanical apparatus might form with eaſe and certainty. After ſketching an outline of a method in my own mind, I imparted my unfiniſhed project to an ingenious friend, being induced to aſk his aſſiſtance by a conſciouſneſs of my own inexperience in the art of writing. This gentleman completed the buſineſs in a ſhort time, by inventing a ſet of diſtinct characters of eaſy execution, and contriving the means of imprinting them upon paper.

Requiſites of the art.

The origin of the invention being now ſtated, the proceſs may be deſcribed in the following manner; including the preparation of the paper, the alphabet, the law of combining the letters, the apparatus, and the proper kind of ink.

Practice. The paper is ruled into ſquare compartments.

The paper is prepared by dividing it into ſeveral ſquare compartments, which will appear in the annexed figure, page 57. The operation might be performed by means of a ruler, but the page may be ſtamped with greater certainty by the writing-frame; which inſtrument will be deſcribed in the ſequel.

The alphabet is formed by placing a dot or a ſtroke in a different part of the ſquare for each letter.

The alphabet is formed by means of two marks only; theſe are a dot, and a ſtraight ſtroke; for each compartment may be conſidered as the ſkeleton of any character the writer wiſhes to make; and it is completed by placing the requiſite mark in its proper place. The letters might be deſcribed ſeparately, but a moment's inſpection of the alphabet given in *Fig.* 1ſt explains the ſtructure of it better than words can do. The ſcheme moreover ſhews, that there are thirteen places where the marks may be fixed, ſo as to form all the letters: to theſe may be added the three characters denoting the end of a word, a capital, and the numerals 1, 2, 3, &c.

Method of combining the letters or of writing.

Since placing a dot or a ſtroke in a ſquare makes a letter, the ſame compartment may form the baſis of ſeveral characters in certain circumſtances; provided the law of combining them be firſt eſtabliſhed.

My

My method conſiſts in ſuppoſing the thirteen places of each compartment to be numbered, as is done in the annexed figure (*Fig.* 2d.) Now if, in ſpelling a word, the place of the former of two adjacent letters be of a leſs denomination than that of the latter, they may both be put in the ſame ſquare; in like manner if the ſecond do not exceed the third in value, this laſt may alſo be inſerted with the other two; and the ſame rule will obtain with the fourth, &c. Letters of the ſame denomination, as o, r, &c. are to be placed in ſeparate ſquares; and every capital muſt have a compartment to itſelf. This way of writing obſerves no proportion betwixt the length of a word and the ſpace occupied by it: the monoſyllable (and) fills three ſquares; which is the number taken up by the triſyllable (inſpection) See *Fig.* 1ſt.

The elementary proceſſes of the art being now explained, we come in the next place to the deſcription of the inſtruments and their uſes.

The writing-table is an oblong board, having two parallel braſs plates paſſing from bottom to top, and containing a ſpace between them, juſt ſufficient to receive a ſheet of paper: they are an inch in breadth, and $\frac{1}{8}$ of an inch in thickneſs, being fixed with their flat ſides to the table. A row of ſhort pins is placed along the middle of either plate: each pin in one row is oppoſite to one in the other, and the diſtance of two adjacent pins in either plate, is exactly equal to the breadth of the writing frame. This inſtrument is an oblong piece of braſs, a quarter of an inch thick: it has a bevil at each end, which parts paſs over the ſide plates of the table, and thereby ſuffer the intermediate parts of the frame to reſt upon the paper. The annexed figure (1ſt) ſhews the ſtructure of the perforated part and the ſize of the ſquares.

The writing table.

The writing frame.

The proceſs of ſtamping the paper conſiſts in covering the underſide of this plate with printer's ink; a ſmall hole being made in either bevil for the reception of two oppoſite pins in the ſide plates; the operation commences by placing the frame on the higheſt pair, it is afterwards ſucceſſively removed to each inferior pair until the whole page is ſtamped and prepared for the pen.

Operation of preparing the paper.

This inſtrument is a ſquare braſs rod, four inches long: each ſide of the ſquare being nearly half as long, as a ſide of

The pen.

of a compartment in the frame. One end of this pen is cut away parallel to one of its ſides, ſo as to form a plate three eights of an inch long and a line in thickneſs: the end of the plate is alſo filed down ſo as to leave at one corner a point, which is parallel to the axis of the rod: this point is uſed in making dots. The other end of the rod is cut diagonally parallel to its axis, thereby forming a priſm, the end of which is an iſoceles triangle. The extremity of this priſm is alſo cut away, ſo as to leave a plate lying in the ſame plane with that deſcribed above: the end of this plane has a ſmall bevil externally, which gives it the edge of a chiſſel; and it is uſed in making ſtrokes. The writing frame always reſts on that part of the page, which you are going to write upon; and the ſides of the compartments together with their angles guide the pen to the proper places: the method of uſing which will eaſily occur to thoſe who are acquainted with the alphabet and apparatus.

Ink. They ſhould however be informed that *printer's ink*, is fitteſt for the purpoſe: it is ſpread upon a cuſhion of ſoft leather, which muſt be touched by the pen, as oft as it is applied to the paper.

JOHN GOUGH.

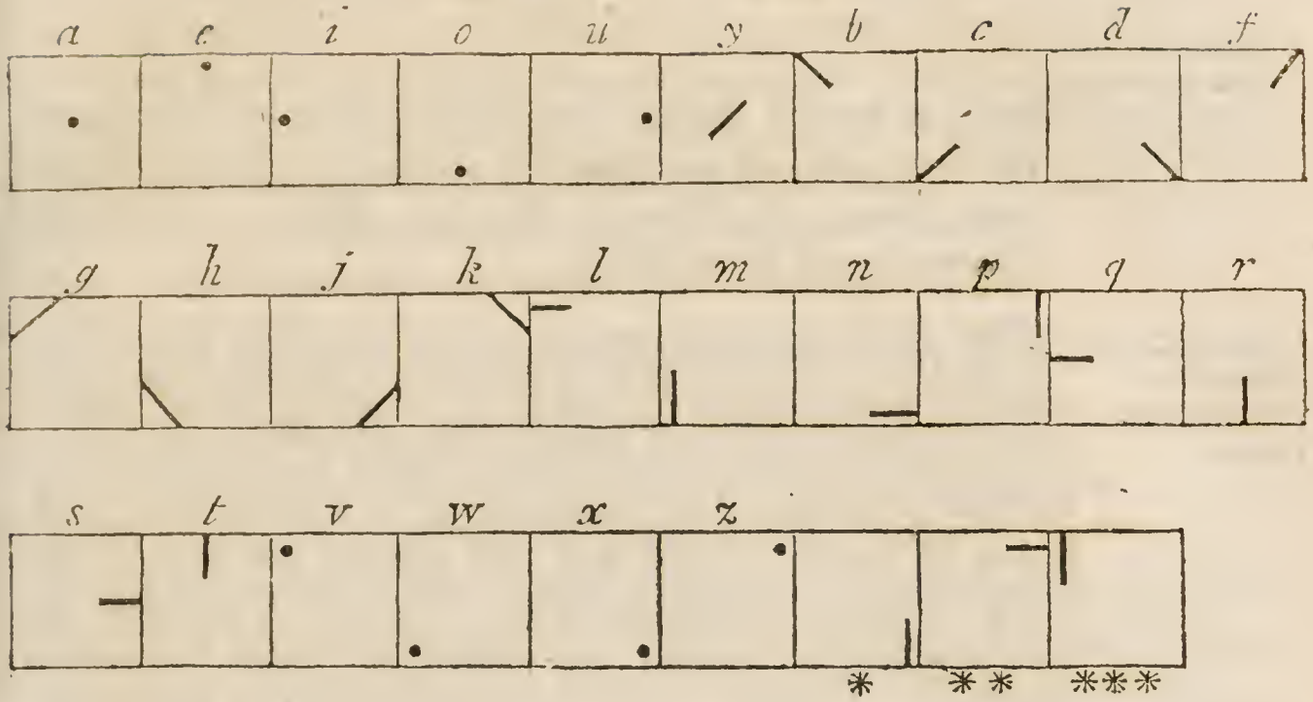
Middleſhaw, Dec. 15, 1803.

(See alphabet, next page.)

P. S. A paper of mine was read Nov. 4th before the Mancheſter Society, in which I endeavour to demonſtrate Mr. Dalton's doctrine of mixed gaſes to be repugnant to the mechanical philoſophy. It alſo proves experimentally that water augments the bulk of air of a given temperature without the intervention of a ſtopple of mercury; from which the non-exiſtence of an aqueous atmoſphere is inferred upon known principles.

ALPHABET.

ALPHABET. *Fig.* 1*ſt.*



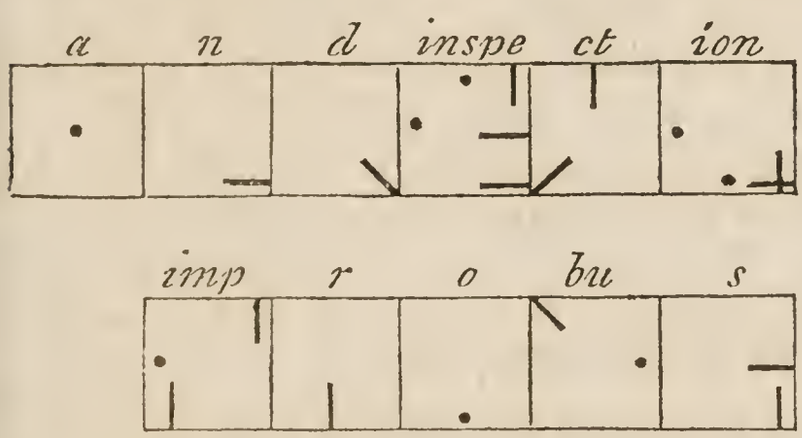
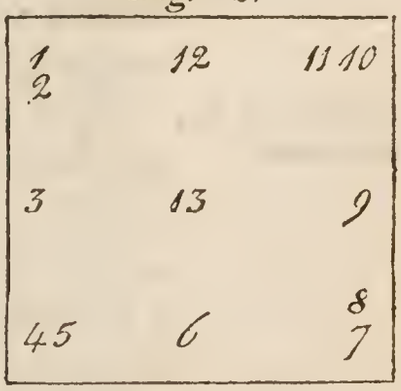
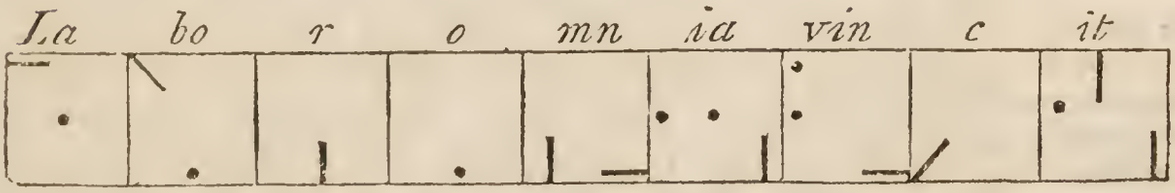


Fig. 2.





Denominations of the Letters.

1	2	3	4	5	6	7	8	9	10	11	12	13
b	l	i	c	m	o	d	n	u	f	p	e	a
g		q	h		r	j		s	k			y
v			w			x			z			

* The ſquare diſtinguiſhed by one ſtar underneath contains the mark to denote the end of a word.

** The ſquare ſo diſtinguiſhed contains the mark denoting a capital letter.

*** The ſquare ſo diſtinguiſhed contains the mark ſignifying a numeral character or number.

XI. *Method*

XI.

Method of uniting Sulphur and Phoſphorus without Danger to the Operator, and an Attempt to explain the Change that then takes Place. By ROBERT BRIGGS, M. D. *Edinburgh. Communicated by the Author.*

Danger of combining ſulphur and phoſphorus.

MR. ACCUM has lately favoured your readers with an account of ſome experiments made on the compound of ſulphur and phoſphorus, ſhowing the great danger of preparing it by the uſual proceſs, but without pointing out a method by which it may be readily formed, and that without the ſmalleſt riſk in the hands of a careful operator. I ſay careful operator, for no preparation of ſulphur and phoſphorus ſhould be handled careleſsly. In order to prevent effects ſimilar to thoſe which happened to Mr. Accum, is the purport of this paper.

Limited combination of ſulphur with phoſphorus under 180°.

If equal quantities of ſulphur and phoſphorus are melted together under water at a temperature not exceeding 180° of Fahrenheit's thermometer, a certain proportion only of the ſulphur unites with the phoſphorus. The remainder is eaſily waſhed away with cold water. This does not hold in higher temperatures: any quantity of ſulphur may then be united.

Qualities of this compound.

Upon examining the compound after it is perfectly cold, it will be found to have a friable texture, ſpungy appearance, and a ſulphur colour. The heat of the hand is ſufficient to melt it, and a temperature of about 112° or nearly ſo will ſet it on fire if perfectly dry. To dry it perfectly, however, is very difficult. When juſt formed, and during its formation, there is a ſtrong ſmell of ſulphurated hydrogen, and a conſiderable quantity of that gas makes its eſcape. After the heat of the compound under water is raiſed to 200° or 210°, the water is decompoſed very rapidly; white fumes, which before filled the empty ſpace of the flaſk, now iſſue out of its mouth in abundance, and if the veſſel is ſhaken by the hand or by allowing the water to boil, a ſudden inflammation and exploſion is the conſequence. The compound of ſulphur and phoſphorus begins to decompoſe the water immediately after the union of the two commences, but, towards the boiling point of water, that decompoſition is very rapid.

If the heat be raiſed nearly to boiling, the water is rapidly decompoſed, white fumes eſcape, and it explodes.

If

If you take the veſſel of the fire juſt when the white fumes have filled the flaſk, but, before they begin to iſſue from its mouth, and place it in a dark room without ſhaking it in the leaſt, the whole ſpace above the water which is filled with the denſe white fumes, has a beautiful appearance, like an aurora borealis. At this time very frequently, though not always, bubbles of phoſphorated hydrogen gas, or rather ſulphur phoſphorated hydrogen gas, if I may be allowed the term, eſcape and take fire on coming in contact with the external air, forming a beautiful white ring of ſmoke riſing to the cieling of the room,—juſt ſuch as happens from the decompoſition of water by means of phoſphuret of lime or potaſh.

The white fumes are beautifully luminous in the dark, and fiery rings ſometimes appear.

Now what is the change produced during this formation? That the water is decompoſed, is abundantly evident. But while the hydrogen of the water eſcapes, what becomes of its oxigen? If the oxigen were alſo allowed to eſcape, we ſhould feel its preſence, from exploſion after exploſion taking place, immediately after it left the water along with the hydrogen, united with as much phoſphorus as is ſufficient to fire the two.

In this proceſs, though the water is decompoſed, the oxygen is not ſet free.

The firing, however, only happens after the ſulphur phoſphorated hydrogen reaches the external air, or when the decompoſition goes on very rapidly from a high temperature, and faſter than the oxigen can be diſpoſed of. From all theſe facts taken together, I believe it muſt be pretty obvious, that the compound which we form of ſulphur and phoſphorus under water, is not of pure ſulphur and phoſphorus, but of their oxides. That is, during the junction of ſulphur and phoſphorus by heat under water, part of the water is decompoſed, the hydrogen eſcapes after engroſſing a ſmall portion of ſulphur and phoſphorus, while the oxigen unites with the ſulphur or phoſphorus, or both, converting them into an oxide of ſulphur and phoſphorus. This will be found farther confirmed from conſidering the nature of this compound. It is very inflammable, much more ſo than a compound of pure ſulphur and phoſphorus formed by another method which I ſhall deſcribe by and by; but which may be converted into an oxide by a very ſimple proceſs, which I ſhall alſo point out; then it becomes one of the moſt inflammable bodies with which I am acquainted. Farther the compound of oxide of ſulphur and phoſphorus, does not decompoſe cold water ſo far as I have been able to perceive, though the contrary is ſaid by Mr. Accum; but it will decompoſe the water if heated.

It is employed in oxiding the baſes.

which oxide is more inflammable than the ſimple compound.

Phoſphorus and ſulphur do not decompoſe water unleſs heated.

Upon

The compound firſt mentioned is of the oxide; and the phoſphorated and ſulphurated hidrogens contain oxigen.

Upon the whole then, I imagine, that, this compound when formed under water, is nothing elſe than a mixture of the oxide of ſulphur and the oxide of phoſphorus, and that phoſphorated hydrogen gas, is not phoſphorus diſſolved in hydrogen gas, but a ſolution of the oxide of phoſphorus in that fluid, and the ſame may be ſaid of ſulphurated hydrogen. It is not ſulphur, but an oxide of ſulphur that is diſſolved.

Beſt method of forming it,

I ſhall now proceed to mention what I conſider as by far the beſt method of forming a junction of ſulphur and phoſphorus, and of afterwards converting them into oxides without riſk or danger.

When I firſt began to examine the nature of the oxides of ſulphur and phoſphorus, the decompoſition of water, and the eſcape of its hydrogen only, at a low temperature, I imagined that I could effect the union of the two much better without the water.

The mixture heated in a phial covered with a perforated bladder.

I took a phial glaſs and filled one third of it with the uſual proportions of ſulphur and phoſphorus, I tied a piece of ſtrong bladder over the mouth of the glaſs, and made a ſmall hole in it with a pin. I expected that after the phoſphorus began to melt, probably it would fire with the ſmall quantity of oxigen of the atmoſpheric air in the phial, before the ſulphur and phoſphorus had joined. I therefore uſed the precaution of a ſmall hole in the bladder to allow the eſcape of the rarefied azotic gas. It did fire, and was followed by a trifling exploſion.

The heat was by this ſuddenly augmented, the union of the ſulphur and phoſphorus formed immediately, and nothing more particular happened. The heat, however, is raiſed ſo ſuddenly that if the phial is large it is very apt to break. I would not therefore recommend this to be tried by a young chemiſt without great caution.

Explanation of Mr. Accum's accident.

Before going farther, I would now attempt to explain the accident that happened to Mr. Gardenas related by Mr. Accum. The compound of oxide of ſulphur and phoſphorus which he was melting with the phoſphorus, had either not been thoroughly freed from water, which it is very difficult to do, and hence the exploſion is eaſily explained on the principles which I have already related; or, allowing that there was no water, the phoſphorus had been fired by the oxigen of the atmoſpheric air in the upper part of the phial, at the very moment of

ſhaking

ſhaking the glaſs. This of courſe would raiſe a quantity of the melted phoſphorus, and at the ſame time oblige him to remove his finger from the mouth of the phial. The highly rarified nitrogen would ruſh out with violence, carrying part of the melted phoſphorus along with it, an exploſion and its effects would be the conſequence.

A pretty loud report may be produced by heating any portion of air in a phial glaſs covered by the finger, on ſuddenly removing the finger.

Experiment in proof. Phoſphorus and ſulphur gradually heated, in a ſmall cloſed veſſel, unite without danger.

This explanation, however, is rendered abundantly evident from what follows. Fill a phial glaſs, or what is ſtill better, a thin glaſs tube ſhut hermetically at one end with ſulphur and phoſphorus. Cork it firmly, and plunge the tube into hot water, the heat of which is to be gradually augmented till it boils. The ſulphur and phoſphorus will melt and unite peaceably. Shake them after taking the tube from the boiling water, that the mixture may be compleat, and no accident will happen.

Not ſo if the heat be ſudden.

It is hardly neceſſary to caution againſt plunging the tube into boiling water at firſt. A contrary plan allows any ſmall quantity of oxigen that may be preſent in the very ſmall portion of air that may remain between the particles of the ſulphur and phoſphorus to be taken up by the phoſphorus before melting. If a compound of ſulphur and phoſphorus be prepared as above directed, having only a ſmall proportion of ſulphur, it has a yellowiſh white colour, is ſolid when cold, and has a cryſtallized appearance. This ſubſtance is more inflammable than phoſphorus, but not by any means ſo much ſo as a dry oxide of ſulphur and phoſphorus. This compound, however, may be eaſily converted into an oxide by the following ſimple proceſs. Set fire to the mixture, ſtill in the tube, by taking out the cork, admitting a little air, and plunging a hot wire into it; allow it to burn only five or ſix ſeconds. It will by this be converted into the oxide of ſulphur and phoſphorus, be found to reſemble in appearance very much a neutral ſalt effloreſcing, ſublime into the neck of the glaſs, and be ſo very inflammable that the inſtant you bring it into the air it catches fire, though the thermometer be at the freezing point.

This compound is leſs inflammable than the firſt mentioned oxide: into which it may be coverted by combuſtion.

Edinburgh, December 15, 1803.

On

XII.

On the beſt Method of aſcertaining the Dip at Sea. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON,

SIR,

The dip of the horizon rendered very uncertain by refraction.

IT is well known that refraction varies ſo much near the horizon as to render the depreſſion or dip very uncertain, whence it ſeems deſirable, in the preſent improved ſtate of navigation, to find the dip by obſervation rather than truſt to the table. An inſtrument ſo conſtructed as to meaſure an angle of 180° would be very convenient for this purpoſe, and the back obſervation in Hadley's quadrant has been propoſed; but the back obſervation is attended with ſo many inconveniences to the obſerver, and ſo little to be depended upon, as to render it nearly uſeleſs. The inſtrument which I have propoſed for taking any angle leſs than 180° ſeems well adapted to this purpoſe *. The diſtance of the two oppoſite horizons at ſea is = 180° + twice the dip; but the motion of the ſhip renders this method very troubleſome and uncertain; for whatever uncertainty attends taking an altitude of a celeſtial object, that uncertainty is doubled in taking the diſtance of the two oppoſite horizons. To avoid this inconvenience, let the meridian altitude of the ſun's upper or lower limb be taken with a ſextant by one obſerver, and at the ſame time, let another obſerver, with an inſtrument ſo conſtructed as to meaſure an angle of 180° take the diſtance of the ſame limb of the ſun, from the oppoſite horizon; the ſum of theſe two angles − 180° is = twice the dip. And as two obſervations of the ſun's altitude are thus taken independently of each other, the latitude will be obtained with greater preciſion than by a ſingle one.

The back obſervation will not correctly ſhew the dip.

It is beſt found by taking the ſun's altitude and its oppoſite ſupplement by two perſons;

or by one perſon with two inſtruments.

The obſervations may alſo be taken with two inſtruments by one perſon.

Let the obſerver begin ſome time before noon, and take the altitude of the ſun's limb. 2. With the other inſtrument take the diſtance of the ſame limb from the oppoſite point of the

* See Philoſophical Journal, vol. iv. p. 218. and vol. vi. p. 219.

horizon,

horizon, then take the firſt inſtrument, and correct the obſervation; and ſo continue to uſe the inſtruments alternately till the ſun ſhall arrive at his greateſt altitude.

If the ſun's altitude were taken, when off the meridian, by two obſervers, in the manner above mentioned, in order to find the hour from noon, it might greatly add to the accuracy of that important problem. This method is recommended in finding the time.

REMARKS.

From the above ſtrictures it appears, that navigation might receive many improvements from an inſtrument that would take in the largeſt angle, and admit of being accurately adjuſted. The latitude and time of the day might then be determined with greater preciſion than at preſent, and the limits of the method of finding the longitude by the lunar theory would be enlarged.

EZEKIEL WALKER.

Lynn, December 22, 1803.

(To be continued in a future communication.)

ERRATUM.

Vol. VI. p. 219. near the bottom, for 120°, read 180°.

XIII.

New Proceſs for fabricating Alum artificially and without the Aſſiſtance of Evaporation. By Cit. CURAUDAU, *Correſponding Member of the Society of Apothecaries of Paris* *.

A REPORT was made to the National Inſtitute in Fructidor of the year 9, by Citizens Guyton aad Vauquelin, on a new proceſs of Citizen Curaudau, for the artificial fabrication of alum. As this report is not generally known, we haſten to communicate it to our readers.

Citizen Curaudau propoſes, in the fabrication of alum, to employ one hundred parts of clay, and five of muriate of ſoda, diluted with a quantity of water ſufficient to give a paſty conſiſtence to the mixture. It is then made into loaves, with Proceſs employed by Cit. Curaudau to make alum artificially. 100 clay and 5 common ſalt kneaded with water; the loaves

* From the Annales de Chimie, Floreal, An. XI. No. 137.

which

are ignited, cooled, pounded, and one fourth of ſulphuric acid water is added by degrees till 10 times the acid.

which a reverberatory furnace is filled, and wherein a briſk fire is kept up for two hours, or until the inſide of the furnace is obſcurely red. The calcination being finiſhed, the clay is reduced to powder and put into a ſound caſk; one fourth of its weight of ſulphuric acid is then poured over it, at ſeveral intervals, agitating it ſtrongly upon each addition. As ſoon as the diſſengaged vapours of muriatic acid are diſſipated, a quantity of water equal to that of the acid is added, and it is ſtirred as before. This produces a combination between the acid, the earth, and the water, ſo rapid, that the mixture heats, ſwells and emits abundance of vapours. Laſtly, when the heat has abated a little, water is continually added until the quantity amounts to about eight or ten times that of the acid.

To the clear decanted liquor potaſh is added.

When that part of the earth which is uſeleſs in the formation of the alum is depoſited, and the liquor become clear, it is drawn off into veſſels or boilers of lead. A quantity of water equal to that of the liquor drawn off, is then poured over the reſiduum, and afterwards added to the firſt: laſtly, a ſolution of potaſh is mixed with the lye, in which the alcali amounts to one fourth of the weight of the acid employed, and it is agitated. If the preference be given to employ ſulphate of potaſh, the quantity uſed muſt be double that of the alcali.

The alum cryſtalizes by cooling.

After a certain time, the liquor by cooling, affords cryſtals of alum, the quantity of which, when the cryſtallization is completed, amounts to three times the weight of the acid made uſe of. This alum is refined by diſſolving it in the ſmalleſt poſſible quantity of boiling water; and it is then as pure as the beſt alum of commerce.

The waſhings employed in a ſubſequent operation.

As the reſidue ſtill retains ſome ſaline parts, Citizen Curaudau recommends waſhing it a third time with a quantity of water ſufficient to extract all the ſalt, and to uſe this inſtead of ſimple water in a ſecond operation; by which means, there will not be any loſs.

The mother-waters uſeful in making pruſſian blue.

Thus, without the aſſiſtance of heat, the greateſt part of the alum formed in the operation is obtained, which is highly beneficial. The author recommends the uſe of the mother-waters, which ſtill contain alum with ſulphate of iron very much oxided, in the fabrication of pruſſian blue, to which purpoſe they are well adapted.

He

He conſiders the fabr ation of alum as particularly advantageous to the manufacturers of pruſſian blue, becauſe they may calcine their clay at the ſame time with their animal matters, without any increaſe of expence. They will have no need in that caſe to add potaſh; and the preſence of iron, inſtead of being injurious, will, on the contrary, be very uſeful. If they are deſirous of fabricating alum for ſale, they may uſe the ſolution of ſulphate of potaſh ariſing from the waſhing of their pruſſian blue, and which is uſually loſt, inſtead of water, to diſſolve the combination of alumine and ſulphuric acid. The reſiduums or refuſe of the diſtillers of aquafortis are equally applicable to the ſame purpoſes: they contain the alumine and potaſh requiſite to the compoſition of alum, and it will be ſufficient to ſprinkle this ſubſtance, reduced to powder, with ſulphuric acid, and to add the neceſſary quantity of water to the mixture, proceeding as has been directed above. The mother-waters of theſe alums are alſo uſeful in the fabrication of pruſſian blue.

This proceſs very beneficial to the makers of pruſſian blue.

Refuſe of the diſtillation of aquafortis may be uſed.

We may alſo obſerve on this occaſion that the refuſe of the diſtillers of aquafortis contains more alcali than is required for the ſaturation of the ſulphate of alumine formed by the clay, and that to obtain the greateſt poſſible quantity of this ſubſtance, it is neceſſary to add an eighth by weight of this earth calcined, according to the proceſs of Citizen Curaudau; and that in uſing about ſixty parts of acid to a hundred of this earth, at leaſt one hundred and eighty parts of very fine alum are obtained.

It contains an exceſs of alcali.

Citizen Curaudau affirms, that he has for a long time prepared alum by theſe different proceſſes with an advantage of more than 25 per. cent. over that of commerce, and that, notwithſtanding the price to which it has now fallen, he ſtill derives from 10 to 12 per. cent. profit. Laſtly, the manufacturers of pruſſian blue, to whom the potaſh would be no expenſe, would at this time gain from 17 to 18 per. cent. by making this ſalt artificially.

Profits from theſe proceſſes (in France.)

From the contents of this report, is is ſeen that the memoir of Citizen Curaudau contains the reſults of experiments made on a large ſcale, highly intereſting to manufacturers and merchants, and that, in this view, it will be uſeful to circulate them through the channel of the ſcientific Journals. We are therefore of opinion, that the claſs will give much credit to

Citizen Curaudau for having publiſhed the reſult of his obſervations, and that it ought to encourage him thus to turn the product of his uſeful labours to the advantage of his country.

Done in the claſs of phyſical and mathematical ſciences, the 11th Fructidor, in the year IX.

Signed, GUYTON and VAUQUELIN.

The claſs approves the report and adopts the concluſions.

XIV.

Letter from Mr. JOHN FARCY, *concerning the great Fiery Meteor of Nov.* 13, *and on other Subjects.*

To Mr. NICHOLSON.

SIR,

Correct date of the appearance of the fiery meteor in November laſt. *It was on the 13th.*

THE great value of your Journal, as a record of facts connected with ſcience, induces me to requeſt you will correct the errors which have unfortunately crept into your account of the meteor of the 13th ult. which is deſcribed, in the ſecond line of the title, and in the firſt line of the account at page 279, to have happened on the *ſixth*; and alſo in the title of the cuts in the next page, where it is deſcribed as an event of the *ſixteenth*. I am much concerned that your friend who ſaw this intereſting phenomenon over St. Ann's church-yard, did not give an eſtimate of its elevation, or of its neareſt approach to the zenith, or of the number of ſeconds during which the light occaſioned by it continued.

Inquiries about its courſe, &c.

I was not ſo fortunate as to ſee it myſelf, or ſince to meet with any ſcientific perſon who did, but on the following evening, I enquired minutely of four or five watchmen of St. Martin's and St. Margaret's pariſhes, who were in different places at the time in their way to their reſpective watch-houſes; I went with each of theſe perſons to the ſpot, and heard their relation of every circumſtance which they ſaw; one of theſe perſons being juſt by the Horſe-Guards within the Park, and another in the front of the Admiralty, agree in fixing the time at about twenty-nine minutes paſt eight by the Horſe-Guards clock; from their compariſon of its height with the ſeven ſtars then viſible (though it was rather hazy) I am inclined to think

that

that it paſſed within about 30° of the zenith, to the South of it, and in a direction from E S E. to W N W. by cauſing theſe perſons ſeparately to count one, two, three, &c. as long as they thought the light laſted, while I looked at my watch, I conclude its light laſted but five ſeconds, which agrees very well with the two or three ſeconds in which it was ſtrong enough to affect perſons in a room, as mentioned by you at page 279.

Greateſt altitude about 60°, and courſe W.S.W.

During the very heavy fall of ſnow on Monday evening the 5th Inſt. as I was returning home from Drury-Lane Theatre, I ſaw at diſtant intervals, four or five ſudden flaſhes of light, which attracted the notice of many perſons near me, and was concluded to be lightning: has lightning been obſerved during the formation of ſnow in the atmoſphere? The air felt rather warm at the time.

Flaſhes of light during a fall of ſnow.

Your candour will, I truſt, excuſe me in lamenting the too conciſe deſcriptions given, at pages 218 and 219, of the ſyphon, and of the ſubſtitute for a fly wheel; it has not appeared evident to me that the ſyphon would act at all, without a ſtop cock below the funnel, *Fig.* 3. In line 7 of page 219 the printer has evidently made ſome miſtake, and what is given below in explanation of *Fig.* 2, is ſo far from evident, that I hope you will at your leiſure give a more detailed account. In page 220, laſt line but one, it ſhould refer to *Fig.* 4 inſtead of 3.

Obſervations upon ſome articles in this Journal.

You will greatly oblige me by giving a place in your Journal to a paper of Mr. Playfair's in the Edin. Trans. vol. V. 1802, containing Theorems on the Figure of the Earth. And to a paper in the Trans. of the Linnean Society, deſcribing an extraordinary appearance of the layers of flint in the chalk ſtrata of the Iſle of Wight. I am in haſte,

Valuable works indicated.

Sir,

Your obedient humble Servant,

JOHN FARCY.

No. 12, *Crown-Street, Weſtminſter,*
Dec. 15, 1803.

REPLY. *W. N.*

Having received Mr. Woulfe's communication late in the month, I gave the account nearly in the conciſe words of the

inventor. I will take an early opportunity of ſpeaking more fully reſpecting it. In Mr. Howard's ſyphon the funnel part may in ſome meaſure be conſidered as a ſubſtitute for the ſide ſuction pipe. In large ſyphons neither will operate unleſs there be a ſtop cock at the bottom of the outer leg: in ſmall inſtruments, ſuch as that in the drawing, the ſtoppage is made by the finger.

The principal intention of a periodical journal being to publiſh in the form of a book ſuch eſſays as from their conciſeneſs might neither be circulated nor preſerved adequately to their merits, it ſeems to follow that larger works muſt for the moſt part be exceptionable from the ſpace they occupy. The excellent treatiſe of Profeſſor Playfair being in Engliſh, and of conſiderable length, has not yet been reprinted in our work, chiefly for the latter reaſon; but I will avail myſelf of the obliging indications of Mr. Farcy to re-conſider both the works he mentions.

XV.

Method of reſtoring rancid Eſſential Oils. *By* J. B. De Roover.*

Proceſs for purifying rancid volatile oils.

DURING my long and frequent experiments on eſſential oils, I have diſcovered a method of reſtoring them, when rancid, which appears to poſſeſs great advantages. The experiment which led me to it is as follows:—

One ſixteenth of ether added to the oil, and left in digeſtion; five pints of alcohol added. Diſtillation on the water bath. Moſt of the oil remained behind.

I mixed one ounce of ſulphuric ether with ſixteen ounces of oil of peppermint ſlightly rancid; I left this compound in digeſtion for twenty-four hours, and then added five pints of ſpirits of the ſame mint. The ſolution was complete. I filtered the liquor and diſtilled it over the water bath. I expected that nearly the whole of the oil would have paſſed over in the diſtillation along with the alcohol and the ether; but, on opening the ſtill, I was much ſurprized to find the greater part of the oil ſwimming on the ſurface of the reſidual fluid. To aſcertain the quantity of oil which had paſſed over, I precipi-

* From the Journal of Van Mons (to whom it was communicated by the author,) V. 107. No. 13.

tated it from its ſolution in alcohol by water, according to my method, and four ounces and a half of oil were ſeparated as tranſparent and colourleſs as water.

Oil was ſeparated from the alcohol by adding water.

I then poured the reſiduum of the ſtill, together with the ſpirit and the water of precipitation, after having ſeparated the oil, into an apparatus over a naked fire, and diſtilled a ſecond time. I had the ſatisfaction to find a ſpirit riſe, the water of which, that came over at the ſame time, depoſited an oil as perfect as before. I repeated this cohobation by pouring the mixture of ſpirits and water on the remaining oil through the neck of the alembic until I had collected in all twelve ounces and a half of oil as fragrant and free from rancidity as the freſheſt and beſt oil of peppermint.

Second diſtillation of all the products.

Cohobation and re-diſtillations, gave a pure oil.

I rectified the ſpirits and judged by ſome trials that it might contain rather more than one ounce and a half of oil.

Some oil in the alcohol.

What remained in the ſtill was a thick adheſive matter of the nature of turpentine. I boiled it with water, and obtained ten gros (drams) of a ſort of dry turpentine ſimilar to reſin.

Reſinous matter in the ſtill.

It is difficult to aſcertain in what manner the addition of alcohol, or ether, contributes to the reſtoration, and in ſome meaſure the regeneration of ſo large a quantity of eſſential oil, which had loſt the greateſt part of its peculiar odour.

Theoretical ſpeculation.

Formerly it would have been ſaid, that the ſpiritus rector, with which the alcohol of peppermint was impregnated, had produced this effect; but now when we know that the ſmell of an eſſential oil does not depend on a peculiar principle, but on a conſtitution proper to the oily compound, we cannot admit that the oil of the alcohol had rendered any particle of the diſaromatized oil odorant; but we muſt ſuppoſe that the ether or the alcohol are in their decompoſition identified with the ſubſtance of the oil.

In fact, the alcohol, and more eſpecially the ether, contains as a conſtituent part hidrogen, which is the baſe of almoſt all the odours. Alcohol, ether, and the eſſential oils, are equally diſpoſed to undergo an alteration in the relative proportion of their principles. There is conſequently no obſtacle to our admitting this concurrence of chemical operation for the reſtoration which I have obtained of the oil of *mint*.

Another circumſtance tends to ſupport this explanation. I at firſt attributed the oil which I obtained in the ſecond operation to the difference of temperature of the bath, and of the naked

Theoretical ſpeculation on the purifying of volatile oils.

naked fire. But the oily products of the ſubſequent diſtillations being all made at the ſame temperature, left me no cauſe for heſitation in explaining the effect by a ſucceſſive re-action of the ether or the alcohol on the hidrogenated oil.

A third circumſtance in confirmation of this opinion is, that the quantity of alcohol appeared to me to be ſenſibly diminiſhed, and that in the ſmell of the oil, which was perfectly pure, I did not find the leaſt trace of that of ether, which is ſo eaſily diſtinguiſhable. It is true that during the repeated diſtillations, the ether might have been diſſipated as well as the portion of alcohol, which conſtituted the difference between the original and ſubſequent quantities of that liquid.

It remains, nevertheleſs, to be explained how a principle of the alcohol, by aſſimilation with the principles of the oil, ſhould produce preciſely the proportional compound which forms the ſpecies of oil on which it re-acts. To this it may be anſwered that the baſe of this oil is only diſpoſed to take that principle which ſhall replace it in its original ſtate.

But even ſuppoſing this explanation to be unfounded, and that the influence of the ether and the alcohol ſhall appear to be merely concurrent circumſtances for producing a ſeparation of the particles of oil and reſtoring its qualities, it is nevertheleſs certain, that the ſame effect has not hitherto been produced by any other means, and that my experiment furniſhes us with a valuable method of reſtoring to commerce an immenſe quantity of eſſential oil, which would otherwiſe be of no value.

Second experiment.

P. S. Since the above mentioned experiment, I have made another trial with an oil which flowed as thick as turpentine, and had no ſmell by which the plant from which it was extracted could be diſtinguiſhed. I mixed four ounces of this oil with two drams of ſulphuric ether, and after four days diſtilled it with pure water. Two ounces and a half of oil came over as clear and as fluid as ſpirits of wine, having the beſt and moſt decided odour of wild thyme. There remained in the ſtill two ounces of reſinous matter of a dark red colour.

In this experiment the ether certainly developed the ſmell of the oil on which it re-acted, or rather it recompoſed the oil by tranſmitting its hidrogenous principle to the oily baſe.

XVI.

Queſtions, 1. reſpecting the Place of the erect Image behind a Concave Speculum; 2. and of the Image formed by a Concavo-convex Mirror which is not Left-handed, and has the Property of revolving on its Axis along with the Mirror, but twice as faſt; and 3. the Figure of the Sky. By a Correſpondent.

To Mr. NICHOLSON,

SIR,

Queſtions propoſed.

I HAVE a general knowledge of the delightful ſcience of optics, but have no pretenſions to an intimate acquaintance with its depths. I have therefore preſumed to requeſt you will either give your own ſentiments on the following difficulties, or lay them before your readers, ſome of whom I hope will give a popular explanation of them.

General ſtatement of the means by which we infer the poſitions and diſtances of viſible objects.

1. We are taught by all the writers on viſion that the diſtance of any object is aſcertained from the divergence of the rays from any aſſumed point on its ſurface, and that as the ſenſe or judgment refers to the point of divergence, we may have a perception of an optical image by reference to that region or part of ſpace from which the rays in their laſt courſe are directed, whether they have in abſolute fact proceeded from this laſt point or not. Thus it is that we ſee an image behind a looking glaſs though the rays have never actually proceeded from that image; but only move in the ſame directions as if they had done ſo. If the mirror be convex, the image will be nearer to its ſurface, becauſe the rays will be more ſcattered and divergent, and ſeem to have come from a nearer point. And parallel rays are ſaid to come from an object infinitely diſtant.

Looking glaſs.

Phenomena of the images in a concave mirror.

Now Sir, my query reſpecting the concave mirror is this:— When I hold my face cloſe to the ſurface, the image and object touch each other. If I recede, the erect image recedes as the rays become leſs and leſs divergent. Every one knows that when I am at the diſtance of half the radius, the rays are returned parallel, and conſequently the image is then infinitely diſtant. When I move farther off, the rays become convergent, and therefore in a certain ſenſe may denote an image farther off than at an infinite diſtance; but I have no wiſh to diſcuſs this point. When I am at the center of the ſphere all is

confuſion, and beyond that point I begin to diſcern the inverted image that hangs in the air, and is formed by the divergence of the rays from their focal interſections.

Apparent receſs and approach of the erect image in a concave mirror.

Such are the facts, but not the whole of the facts. For I am very ſure, that after the firſt receding of the erect image, it comes to a place where it is ſtationary, and then inſtead of remaining at any ſituation that may be called an infinite or more than infinite diſtance, *it comes forward again* and continues to advance till it is loſt in confuſion.

Difficulty ſtated. Why does the image advance?

Perhaps I may not have expreſſed myſelf with all the clearneſs I could wiſh; but I think I have ſhewn a difficulty and the reaſon why it puzzles me. The true ſcience of nature muſt be adequate to the explanation of all phenomena, or at leaſt it muſt not be contrary to any one of them. What is the ſolution of this?

Optical mirror at Merlin's.

2. There is an optical mirror at Merlin's Exhibition in Hanover-ſtreet, in which you ſee your face perpetually changing from one diſtortion to another, ariſing from its greateſt length varying through all its ſeveral diameters. So that the face becomes abſurdly long, crooked, broad, &c. If you move farther off, the face has its natural aſpect, but is ſeen turning round its center and ſucceſſively aſſuming the erect and inverted poſitions, and every intermediate degree of obliquity.

Singular diſtortions, and revolution of the image.

A poliſhed button of the ſame figure.

While theſe phenomena, produced by an apparatus concealed in a box, employed my occaſional reflections, I happened to ſee a poliſhed metallic button in the ſhops, which upon examination proves to be a mirror of the ſame kind as the larger one uſed at Merlin's. If it be conſidered in the direction of one of its diameters, it is a concave ſpeculum; but in the direction of another diameter at right angles to the former, it is convex: and the radius of each curvature is about one inch. It is the ſame figure as a turner might produce in the lathe by uſing a chizel with a circular edge of one inch radius to ſcoop out a portion of a revolving piece till its ſmalleſt diameter, or calliper, became equal to two inches. Its effects are,

Particular deſcription of the images produced by the button.

When the eye is brought very near the poliſhed ſurface, it appears elongated in the direction of the diameter or chord, or of the concavity, and ſhortened in the diameter at right angles to that diameter, and if the ſpeculum be turned on its axis, the image is affected with the ſtrange undulation before mentioned, and undergoes all the variations twice in one revolution

Undulation.

volution of the mirror. But when the eye is at a conſiderable diſtance, a ſmall perfect image is ſeen, which when the diameter of concavity is horizontal, is erect but not left handed; that is to ſay, if you move your right hand, the image alſo moves its right hand, inſtead of the left, as happens in a convex and a plain mirror; when the diameter of concavity is vertical, the image is inverted, but ſtill right handed; and in the other poſitions of the mirror it takes every poſſible ſituation of obliquity, making two complete revolutions for one of the mirror itſelf. The image is larger the nearer the object, and the erect image is a little broader than the inverted; this deformity being more conſiderable the larger the image.

Small revolving image, not left-handed.

Now Sir, my ſimple queſtion is, *Where is this image?* Is it before the mirror, according to the property of the convex, or behind it, according to that of the concave. I thought to have diſcovered this by the ſolar focus, or by applying a magnifier to the ſmall image, but neither method anſwered my purpoſe, and nothing within the ſmall extent of my knowledge has enabled me to ſolve it by reaſoning.

Where is this image? before or behind the mirror.

3. The enlarged appearance of the moon near the horizon, and the flat vault preſented by the concavity of the heavens, have been explained by various alluſions to the diminiſhed light after paſſing through a portion of the atmoſphere, and its direction over a long row of objects, ſuch as trees, houſes, &c. Something ſeems to be wanting in theſe accounts. Neither the ſun or moon are ever ſuppoſed to be more diſtant or larger when ſeen at conſiderable elevations through clouds or miſts; and the ſky ſeems flat, and theſe luminaries enlarged, whether the proſpect be over woods, lawns, naked plains, or the calm ſea. Beſides which, I have no heſitation in inſiſting that the ſky appears loftier on ſome days than on others, and even at the ſame inſtant of time on different ſides. If this be no error, what are the cauſes of the appearance or the conviction produced in our minds?

Flatneſs of the ſky and horizontal moon.

Not the ſame at all times, &c.

I am, Sir,

Your obliged reader from the firſt,

R. B.

I believe my correſpondent would have vanquiſhed his difficulties, with a little more conſideration. If the explanation be not given by ſome of my correſpondents, I ſhall endeavour to give the replies he requeſts in our next. W. N.

A New

XVII.

A New Method of preparing the pure Gallic Acid.

By M. L. SCHNAUBERT *.

Preparation of gallic acid.

FOUR ounces of finely powdered galls are infuſed in water ſlightly alkalized with potaſh; the infuſion which is of a very deep colour is filtered, and a ſolution of nitrate of tin is dropped into it, until no more precipitate is formed. The free acid of the liquid is then ſaturated with potaſh, taking care not to add the alkali in exceſs, and the precipitate is ſeparated by filtration. The filtered liquid is then precipitated by acetite of lead, the weight and concentration of which is known. A greyiſh white precipitate is thus obtained, which is to be digeſted with diluted ſulphuric acid, the proportion of the acid contained in which, muſt be to the concrete acetite of lead as one to four. After a ſufficient digeſtion, the liquid is carefully filtered and evaporated, in order to afford cryſtals. If the gallic acid thus obtained, ſhould contain a ſmall quantity of ſulphuric acid, it may be deprived of it by digeſtion, with gallate of lead.

In order to be certain of the quantity of ſulphuric acid neceſſary for the decompoſition of a given quantity of gallate of lead, I previouſly diſſolved four ounces of acetite of lead in ſixteen ounces of water, and diluted one ounce of ſulphuric acid in four ounces of water. By mixing theſe two liquids to afford the perfect precipitation, the requiſite proportion was eaſily found, I muſt here obſerve, that it will be better to keep the gallic acid in a liquid ſtate than to reduce it to cryſtals, becauſe evaporation always renders it more or leſs brown, I am firmly perſuaded that the gallic acid poſſeſſes the property of becoming brown by the action of light, at leaſt, if in contact with the atmoſphere. For if gallic acid be ſublimed perfectly white, and then diſſolved in water, it will be found after ſome time to have undergone this kind of alteration. M. Bucholt has made a ſimilar remark relative to the ſame fact.

* Tromſdorff's Journal der Pharmacie, vol. ii. p. 61.

SCIENTIFIC NEWS.

Reward of Twenty Pounds for the Artificial Production of Palladium.

THE following is a copy of a paper received by me under cover, by the two-penny poſt. It is written in the ſame hand as a note which covered a ſmall piece of the palladium mentioned to have been received by me laſt Midſummer. (See Philoſ. Journal, June, 1803. Vol. v. p. 136.) Upon inquiry, I find, that Mrs. Forſter has received the ſum of 20 £. with inſtructions conformable to this paper. This original is cut indent-wiſe on the margin, and has part of a manuſcript flouriſh or paraph on each border, but no ſignature.

(COPY.)

December 16, 1803.

SIR,

AS I ſee it ſaid in one of your Journals, that the new metal I have called palladium, is not a new noble metal, as I have ſaid it is, but an impoſition and a compound of platina and quick-ſilver, I hope you will do me juſtice in your next, and tell your readers I promiſe a reward of 20 £. now in Mrs. Forſter's hands, to any one that will make only 20 grains of real palladium, before any three gentlemen chymiſt's you pleaſe to name, yourſelf one if you like.

It is inſiſted that palladium cannot be formed by art; and a reward is offered for any proceſs to that effect,

That he may have plenty of his ingredients, let him uſe 20 times as much quickſilver, 20 times as much platina, and in ſhort of any thing elſe he pleaſes to uſe: neither he nor I can make a ſingle grain.

Pray be careful in trying what it is he makes, for the miſtake muſt happen by not trying it rightly.

My reaſon for not ſaying where it was found, was, that I might make ſome advantage of it, as I have a right to do.

If you think fit to publiſh this, I beg you to give the names of the umpires, as I have deſired Mrs. Forſter to keep the money till next Midſummer, and to deliver it only in caſe they can aſſure her that the real metal is made by a certificate ſigned by you, and by them, on this check.

I hope a little bit of whatever is made may be left with Mrs. Forſter.

Letter

Letter from Lalande on the Meaſure of a Degree of the Earth in Lapland *.

The meaſure of a degree in Lapland by Maupertuis, &c. ſuſpected of error.

ASTRONOMERS have long ſuſpected, that there was ſome error in the meaſure of a degree of the earth taken in 1736 in Lapland, by Maupertuis, Lemonnier, Outhier, and Celſius, becauſe this meaſure was greater than it ought to have proved according to all others. Mr. Melanderhielm has had the good fortune to obtain a repetition of this meaſurement. He informs me, that Mr. Swanberg, and three other Swediſh aſtronomers, have found the degree to meaſure 57209 toiſes in the latitude of 66° 20′, which are 196 toiſes leſs than by the French meaſurement, and gives for the flattening of the earth $\frac{1}{313}$. This agrees better with the other compariſons, and teaches us, that the figure of the earth is not ſo irregular as was ſuppoſed, from the degree meaſured in the north.

Re-meaſured lately by Swanberg, &c. who found it to be 57209 toiſes, or 196 leſs.

The flattening of the earth 1-313th.

Mechain gone to meaſure a triangle terminating at the baleario iſles.

Mr. Mechain ſet off on the 26th of April for Spain, whither he is gone to meaſure a triangle of 93000 toiſes, terminating at the Balearic iſlands, which will complete the grand and important meaſurement of the meridian, executed a few years ago by Meſſrs. Mechain and Delambre. He is accompanied by Meſſrs. Le Chevalier and Dezauche, and his youngeſt ſon. This meaſurement is very difficult, but no perſon is more capable of ſurmounting difficulties than Mr. Mechain, and we ſhall have the 54th degree in the middle of the whole arc meaſured by Frenchmen.

New metal.

DESCOTILS has diſcovered a new metal in the ore of platina, it is preſumed that palladium may be an alloy of this new metal with mercury.

A blue equal to ultramarine.

Thenard has diſcovered a blue equally beautiful, and as fine as that of lapis lazuli, or ultramarine.

Prize of galvaniſm deferred.

The National Inſtitute has declared, that the galvanic prize will not be given this year.

Theſe three articles are from the Journal of Van-Mons.

* Journal de Phyſique, May, 1803, p. 400.

Letter

Letter from Profeſſor PROUST *to J. C. Delamétherie on a dangerous fulminating Powder* *.

IN one of my late lectures, I made an experiment that might have been attended with ſerious conſequences to ſome of my auditors. Dangerous experiment.

It is, perhaps, no exaggeration to ſay, that the mixture of oxigenated muriate with arſenic, takes fire with the rapidity of lightning.

I am accuſtomed to compare the quickneſs with which different kinds of powder burn, by ſetting fire to them in cannons of equal diameter, paſſing each through a piece of cork placed in water, ſo as to ſink in it almoſt their whole length. Mode of comparing exploſions with that of gun-powder.

In this manner I was deſirous of burning the arſenical mixture, by the ſide of an equal quantity of gun-powder, in order to compare their flame and continuance; but I had ſcarcely time to draw back a little after ſetting fire to the two cannons, when the firſt exploded and burſt, breaking the glaſs veſſel, and throwing about the water in all the radii of the ſurrounding ſphere. Though the mouth of the tube and the eſcape of the flame were perfectly free, the cannon was burſt from top to bottom, and ſpread out as flat as a card. A mixture of oxigenated muriate and arſenic burnt with the rapidity of lightning, burſt the cannon that contained it, rendered it quite flat, and threw about the water in which it floated in all directions.

This powder is ſo violent in its effects, that I conceive it would be very dangerous, to attempt to make any uſe of it. Its effects are too dangerous for uſe.

If two long trains be made on a table, one of gun-powder the other of this mixture, and they be in contact with each other at one end, ſo as to be fired at the ſame time, you will ſee with great ſurprize, that one diſappears like a flaſh of lightning, while the other ſeems to burn with extreme ſlowneſs. A long train of it diſappears like lightning.

Letter from a Correſpondent on the Expediency of converting Foreign Weights and Meaſures into Engliſh.

To Mr. NICHOLSON.

SIR,

THE pleaſure which your readers receive from the peruſal of your Journal is ſo much abated by the obſcurity ariſing from the names of the French weights and meaſures, that I

* Journal de Phyſique, May, 1803. p. 394.

wiſh

wiſh to ſuggeſt to your conſideration, that a reduction of them to Engliſh terms, would be to you comparatively a ſmall labour, in proportion to what it would be to the generality of your readers individually.

Your labours tend to the uſeful purpoſe of communicating ſcience and general knowledge to the public of this country. To facilitate the acquirement of ſuch knowledge, it ſhould be communicated in the terms moſt eaſily intelligible to the people; not in ſuch terms as require algebra, or a proceſs in the Rule of Three, to give a preciſe idea of every meaſurement. As your Journal becomes more extenſively circulated, there ariſes a ſtronger claim upon you from the public, in favour of the many who are too indolent, too buſy, or too ignorant to expound for themſelves this ſpecies of hieroglyphics.

I am, Sir,

Your conſtant READER.

ANSWER.

The rule propoſed to be followed in all our tranſlations, though perhaps not preciſely, and on every occaſion, adhered to, is, that wherever the foreign denominations of weight or meaſure are mere indications of the proportional quantities, (and may be conſidered as pounds, ounces, or grains, or as yards, feet, or inches all through,) the original names are uſed without embarraſing the ſubject with any reduction into fractional quantities: In every other caſe the reduction is, or ought to be made.

While I expreſs my acknowledgments to this reader for his advice and remarks, I beg leave to ſay, that I heartily ſubſcribe to his opinions as to facilitating the communication of knowledge, by the ſimpleſt methods. It is my wiſh in the ſide notes, and by every other means, to ſhorten the time, and aſſiſt the inquiries, of thoſe whoſe other duties forbid the indulgence of deep or continued reſearches.

W. N.

ACCOUNT

ACCOUNT OF NEW BOOKS.

Accum's ſyſtem of chemiſtry.

A Syſtem of Theoretical and Practical Chemiſtry, in Two Volumes with Plates. By FREDERICK ACCUM, *Teacher of Practical Chemiſtry, Pharmacy and Mineralogy, and Chemical Operator to the Royal Inſtitution of Great-Britain. Octavo, Two Volumes, 733. and full Index and Contents, with Five Copper Plates.*

THIS work which, is dedicated by permiſſion to the managers of the Royal Inſtitution, and patronized by a very reſpectable liſt of ſubſcribers, is drawn up for the uſe of ſuch as are unacquainted with chemical ſcience. The author, therefore, begins by deſcribing ſuch experiments as are beſt adapted to exhibit the general nature of chemical action. The laws of affinity, and alſo thoſe which govern the phenomena of heat and light, are ſtated by the method of ſyntheſis, and illuſtrated by experiment. And theſe are ſucceeded by the claſſification of bodies; namely, firſt the principles undecompoſed, then the characteriſtic properties of the gaſes, and theſe are ſucceeded by the metals, alcalis, earths, &c. The diſplay of theſe, as to their properties and habitudes, by an orderly combination of general narrative and reaſoning, with particular facts, experiments or, operative proceſſes, conſtitute the buſineſs of the preſent work, which will doubtleſs prove acceptable to the numerous cultivators of the ſcience of chemiſtry.

Accum's analyſis of minerals.

A practical Eſſay on the Analyſis of Minerals, exemplifying the beſt Methods of analyſing Ores, Earths, Stones, Inflammable Foſſils, and Mineral Subſtances in general. By FREDERICK ACCUM, *Teacher of Practical Chemiſtry, Pharmacy, and Mineralogy. London, 12mo.* 183 pages. 1804.

THIS active and indefatigable lecturer, has been requeſted by his pupils and auditors, to draw up a ſet of conciſe directions, to enable any perſon not intimately acquainted with analytical chemiſtry, to examine ſuch unknown minerals as he may meet with, and readily aſcertain their compoſition.

In

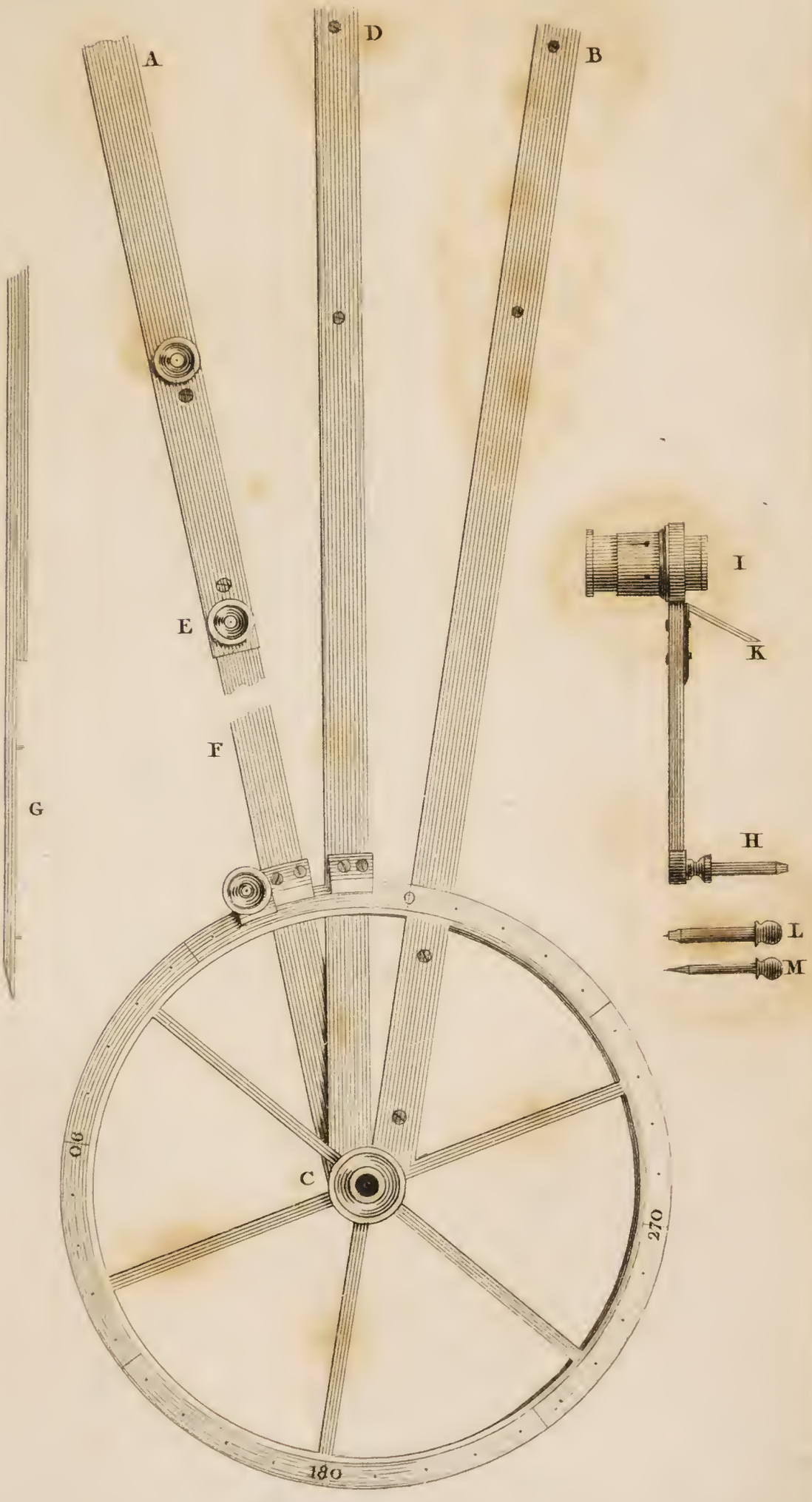
In the accompliſhment of this object he has exceeded his firſt intention, and has not only given clear, and rather comprehenſive inſtructions for analyſing all the different genera, and principal ſpecies of the bodies mentioned in the title, and commonly met with, but has alſo given examples of analyſis of the rarer or leſs abundant mineral bodies.

Boſſut's hiſtory of mathematics.

A General Hiſtory of Mathematics, from the earlieſt Times, to the Middle of the Eighteenth Century. Tranſlated from the French of John Boſſut, Member of the French National Inſtitute of Arts and Sciences, and of the Academies of Bologna, Peterſburg, Turin, &c. To which is affixed, a Chronological Table of the moſt eminent Mathematicians. 8vo. 566. *p. London,* 1803. *One Vol. Octavo.*

THIS juſtly eſteemed work is well tranſlated under the inſpection of Mr. Bonnycaſtle, of the Royal Military Academy at Woolwich, who has added an introductory Preface, and the Table mentioned in the Title Page.

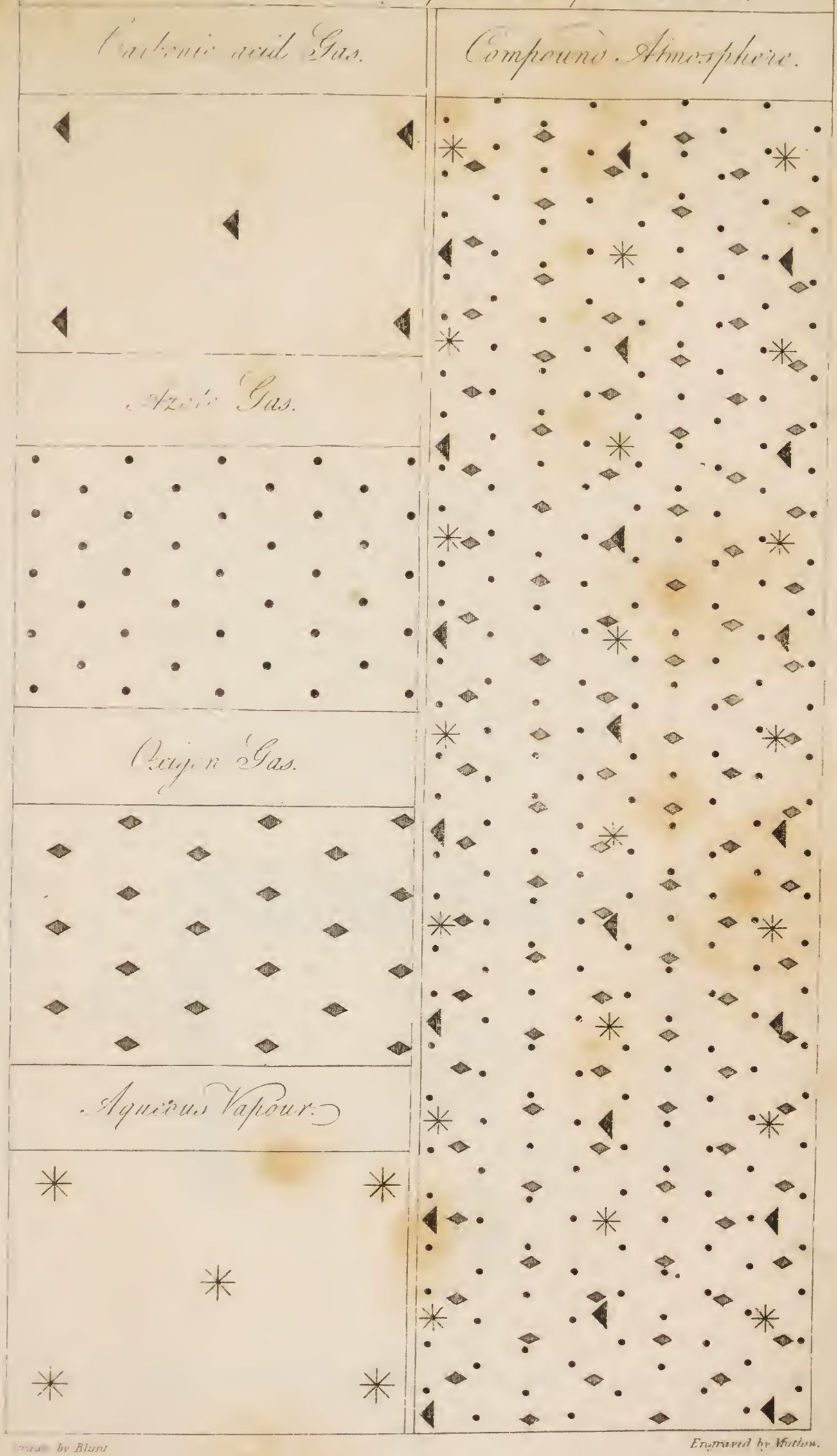
Station Pointer by Joseph Huddart Esq.r F. R. S.



Drawn by Blunt

Engraved by Mutlow

Mr Dalton's Theory of the Gasiform State.



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Determination of Positions, by observation of three known objects.

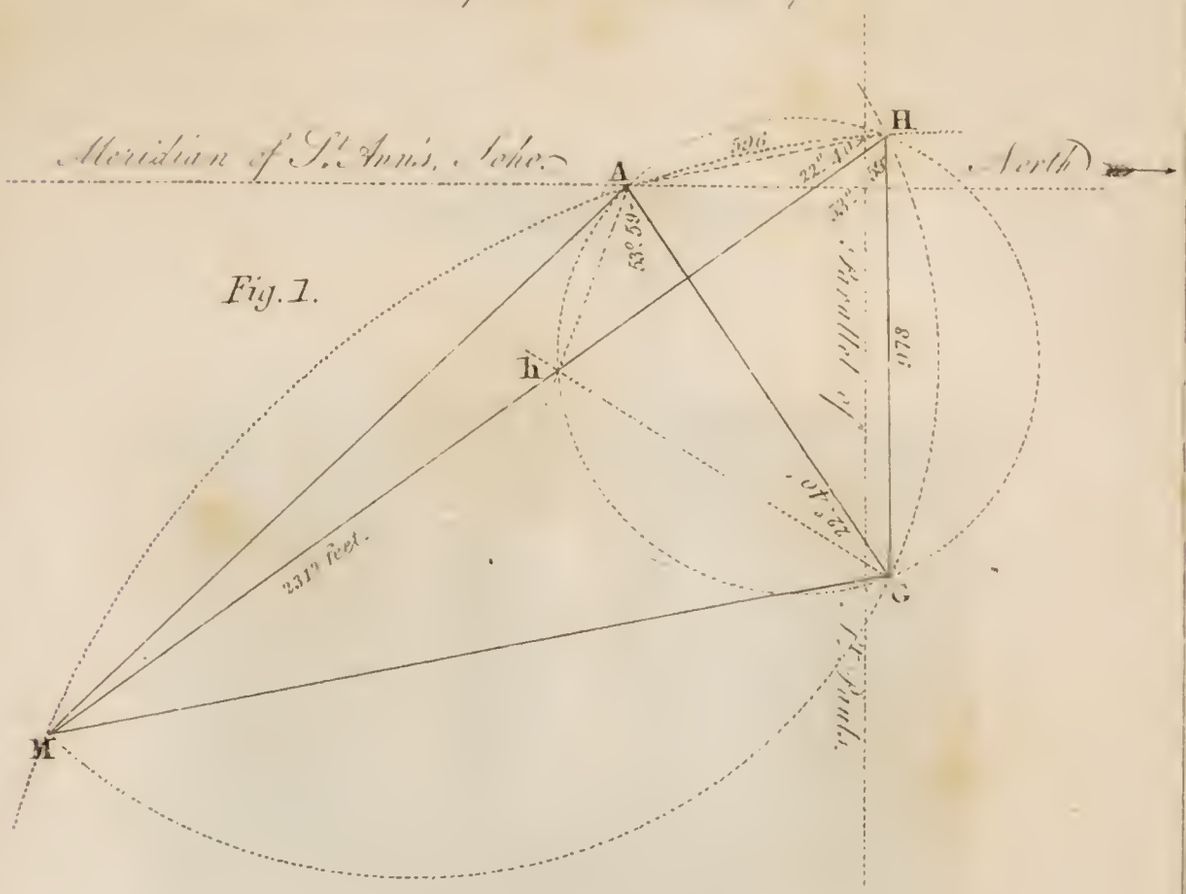
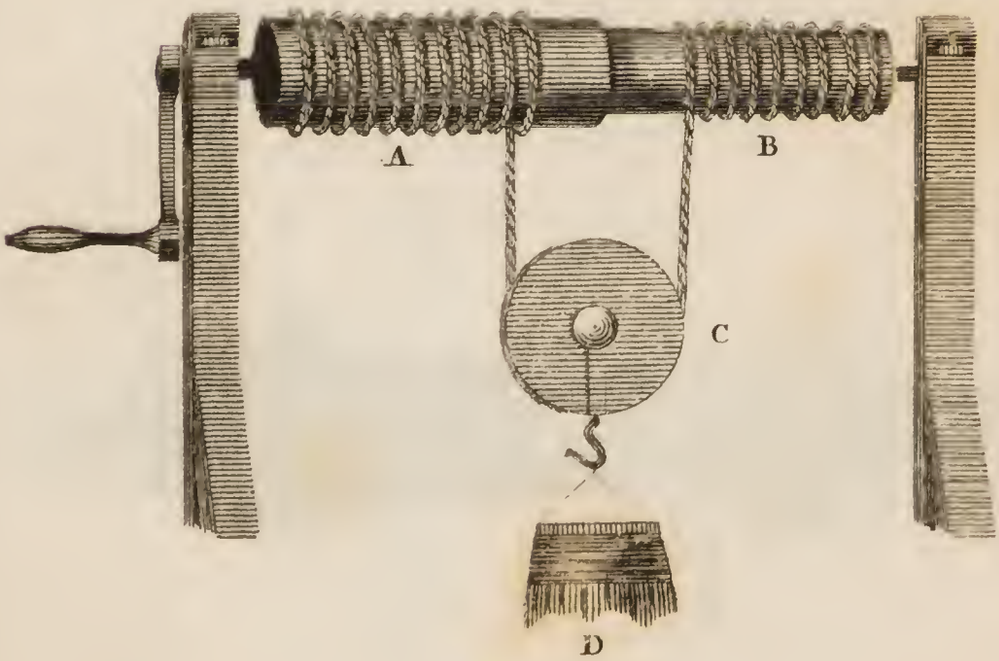


Fig. 1.

Winch & Barrell, (double)

Fig. 2.



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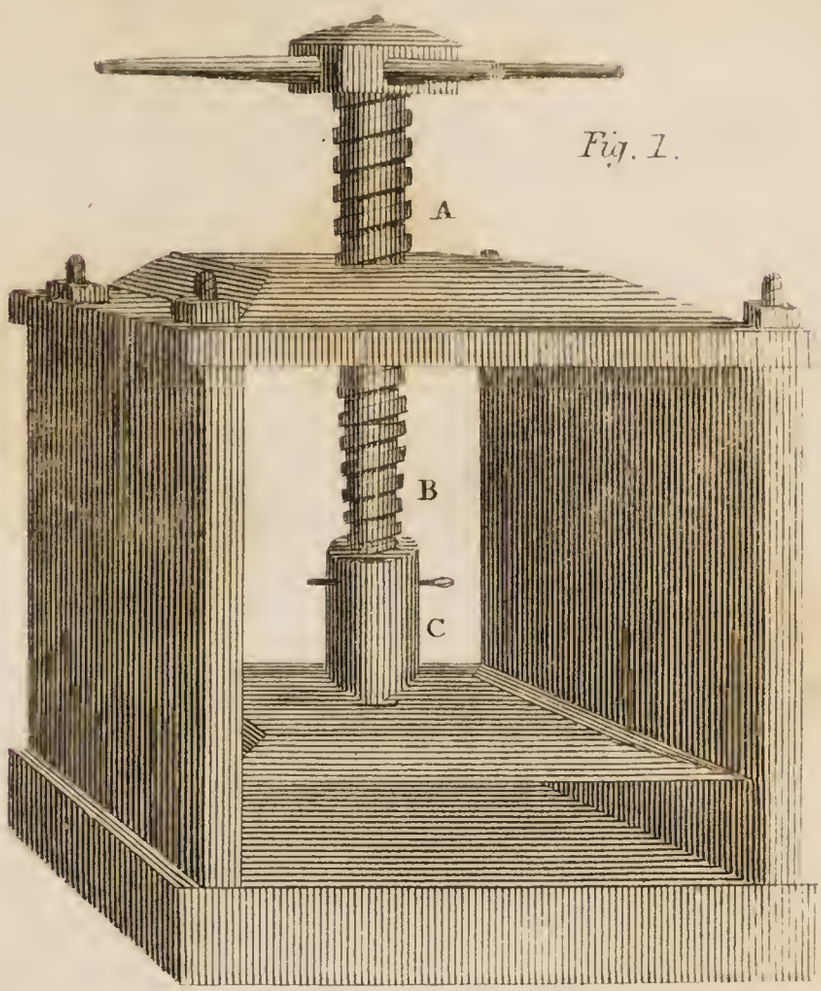
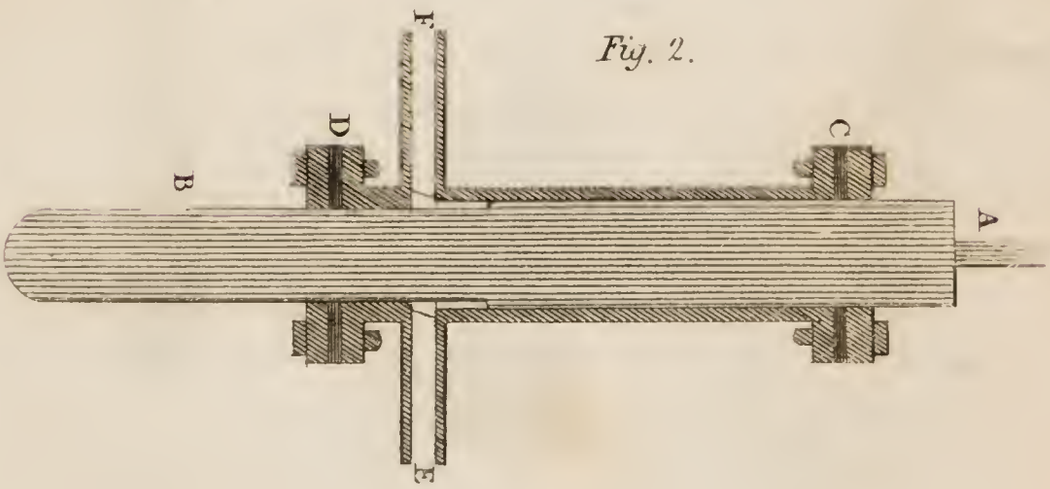
Increase of power by a Double Screw.

Fig. 1.



Pump with double Piston.

Fig. 2.



Drawn by Blunt.

Engraved by Mather.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1804.

ARTICLE I.

Description of an Apparatus, by which the Effect of Atmospheric Pressure in supplying Worm-Tubs and other Vessels with Water on the Syphon Principle, may be secured against any Interruption to be caused by the extricated Air. In a Letter from Sir A. N. EDELCRANTZ, *Counsellor of the Chancery, and Private Secretary to the King of Sweden, Member of the Swedish Academy, &c. &c.*

To Mr. NICHOLSON,

MY DEAR SIR,

Introduction: respecting former endeavours to supply worm-tubs, &c. with water by the action of the syphon.

ON my late return from an agricultural tour in Scotland, I find you have in your excellent Journal, honoured me with the mention of some ideas of mine, respecting the application of syphons to raise water, with less expence than usual, for coolers in distilleries, condensers of steam-engines, &c. The use of syphons is so well and so generally understood, that though the most obvious things do not always present themselves first to the mind, it would have been surprizing if the same notion had not likewise occured to others. In fact, I have since learned, that a person in America, more than twenty years ago, obtained a patent for using the syphon to cool the worm-pipes in distilleries, and though I neither know his name, nor the specification of his invention, I presume it to

be ſimilar to what I have propoſed. In addition to which, the ingenious contrivance of Mr. Howard, publiſhed in a later number of your Journal, is alſo founded upon the ſame hydroſtatical laws. But whatever may be the caſe in reſpect to theory, no practical uſe ſeems yet to have been made of it, owing moſt probably to unforeſeen difficulties in the execution.

The chief impediment lies in the extrication of air which ſtops the current.

The principal of theſe, in my opinion, as was mentioned in the former memoir upon this ſubject, is the extrication of air from the water contained in the cooling veſſel; which air being expanded, partly by its own elaſticity under a diminiſhed preſſure, and partly by the heat, is accumulated in the upper part of the cooler, and interrupts the two columns of the ſyphon, ſo as at laſt to ſtop the motion entirely. For though it may poſſibly happen, by the negligence of workmen, that the worm itſelf may not be ſufficiently ſound to exclude the external air preſſing into the cavity, and introducing itſelf that way into the cooler, as is ſtated in the experiment of Mr. Howard; yet I am perſuaded that in moſt inſtances, this cauſe may be taken for the real one, inſtead of the former, which can never fail to act in proportion to the perpendicular height of the cooler, and the heat communicated by the worm.

Not probable that this air comes from without.

As it is impoſſible to obſerve by what actual means the air is produced, unleſs the whole apparatus were of glaſs, ſuch a miſtake may eaſily be made. In fact, if metallic pipes could not be made air-tight, even in a bended form, the exhauſtions in air-pumps and ſtill more in great condenſations * would be attended with almoſt inſuperable difficulties.

Plan for extracting it.

For extracting the air diſengaged from the water itſelf, during the action of the ſyphon, which in a few hours time would neceſſarily interrupt its motion, I have employed a few hours of leiſure conſtructing the following means, which I beg leave, Sir, to communicate to you; without pretending to claim a greater portion of your attention than their remote utility may appear to deſerve.

* In the experiments I made ſome years ago, with an *apparatus* conſtructed *for condenſing different gaſes and for producing a ſtronger artificial cold, than by any means employed before*, I compreſſed air in braſs pipes more than to $\frac{1}{25}$ of its original volume, which if we ſuppoſe the cooler to be 15 feet high, is more than 50 times the difference between the external and internal preſſure of air in the cooling worm of the new apparatus.

On

On the top of the cooling veſſel A, *Fig.* 1. *Plate* V. let a ſmall veſſel B, called the *air-veſſel*, be placed, communicating with A by a cock. With the top of B let another veſſel C, called the *water-veſſel* be connected, having a cock C. Theſe cocks have two parallel openings each, one for letting out air, the other for letting in water at the ſame time to take its place. Suppoſe theſe three veſſels to be filled with water and the ſyphon in action, the cock B open and C ſhut; then all the air produced in the cooler will riſe through B and fill the air-veſſel; and in the mean time the water will deſcend. The air-veſſel being filled with air, let the cock B be ſhut, and C opened; then the water from C will deſcend, and the air will eſcape into the atmoſphere. By this alternate motion of the cocks, the air diſengaged within the worm-tubs will be continually extracted, as long as any water remains in C.

A veſſel filled with water, with a cock at top and another at bottom, is connected with the upper part of the worm-tub. When the lower cock only is opened the extricated air aſcends, and water deſcends. When the upper cock only is opened, the air eſcapes, and other water fills in from above.

The principal object then muſt be firſt to ſupply the water in C, either by manual labour, or by a part of the mechanical force employed to fill the artificial reſervoir for the lower branch of the ſyphon; and ſecondly, to direct the alternative opening of the cocks, by a ſelf-regulating mechaniſm; which may be effected in the following manner.

The upper water muſt be ſupplied; and the cocks muſt work without attendance.

Fig. 2. *Plate* VI. repreſents a ſection of the *air-veſſel* B, B is the air and water-cock, communicating with the worm-tub or cooler below. A is the other cock for the ſame purpoſe, communicating with the water-veſſel C. A and B being connected by a metallic rod K L, their motions are ſimultaneous, and the perforations are ſo placed, that when the two holes of one cock are open, thoſe of the other are always ſhut. E is a ball of lead or other weight, fixed on the lever G U, connected with the quadrant F G, and the lever G H, the whole turning through one quarter of a circle round the centre U; *m n* is a part of a cog-wheel fixed to and ſliding along the quadrant G F, acting upon the teeth of the cock A, and forcing it, together with the cock B, one way or the other, as itſelf is moved by the levers U F or U G, either in the direction from F to G, or from G to F. This alternate motion is produced by the floater P O Q R, (of which a plan on a diminiſhed ſcale, is repreſented in *Fig.* 3.) having in its middle and hollow part a kind of fork O, through which paſſes the metallic rod H I, connected with the lever H F, at the end of the quadrant G F. This floater in its aſcenſion

The cocks are worked by a float: which when at loweſt ſhuts the under and opens the upper cock; and when at higheſt does the contrary. The action is made ſudden by a tumbling weight.

lifts the little ball H at the end of the lever, and conſequently the weight E; and in deſcending it acts upon the ball I, and forces the weight E in the contrary direction. A ſide view of this apparatus is preſented in *Fig.* 4. where the double openings in the cocks A, B, and the manner of fixing the weight E with its levers, are more diſtinctly ſeen.

In *Fig.* 2. the air-veſſel is ſuppoſed half filled with air tranſmitted from the cooler. The cock B is ſhut, and the cock A open, admitting water in place of the air. The floater is aſcending, and when the fork O touches the ball H, the weight E is lifted, the quadrant begins to move from E to G, but the ſliding part *m n* remains in its place till the weight E, having paſſed the perpendicular and falling over the other ſide, brings the point F in contact with *n*, and forces at once both cocks in the contrary direction; by which A being ſhut and B open, the air from the cooler riſes, and the water deſcends. The floater conſequently ſinks till it preſſes upon I, drawing the weight E in its firſt ſituation, and turning the cocks again. This proceſs will evidently continue to alternate.

Every mechanical reader will ſee the neceſſity of turning the cocks in this apparatus by a ſudden ſtroke. For if the floater were to act directly and conſtantly upon them; there would be a certain moment when both would be ſhut, and conſequently the motion would ceaſe.

Another method by chains.

Inſtead of the ſlider *m n*, the ſame effect may be produced by two chains, (*Fig.* 5.) on the ends of the levers F G, connected with the cock A; the other apparatus remaining as deſcribed above. This method may even in ſome reſpects appear more ſimple; but I have deſcribed the other partly becauſe I apprehend the contrivance to be new, and partly becauſe the uſe of chains may in ſome caſes be liable to objections.

An air-pump would be the ſimpleſt organ.

But I cannot avoid remarking that the moſt ſimple means of all, and perhaps the beſt, on a ſmall ſcale, would be an *air-pump* fixed on the top of the cooler, and connected with the pump that ſupplies the artificial reſervoir. And though the force required for extracting the rarified air, muſt be in proportion to the perpendicular height of the ſyphon; yet it cannot be more than what is required to raiſe a quantity of water neceſſary to fill the place of the air, as in the former

apparatus. When it has been found by experiments, how much air is extricated from a given quantity of water, running with a known velocity, under a given diminution of preſſure and heat; the ſize and power of the air-pumps may be determined ſo as to act with full advantage, as long as theſe circumſtances remain unvaried. But if they happen to change, the air-pump will either work too much, and expend an unneceſſary force, by extracting water after the air is exhauſted; or too little, by extracting the air more ſlowly than it is produced. On the contrary, it ſeems that the floater, if the veſſels and cocks be of ſufficient ſize, can never deliver more or leſs than is exactly required.

This conſideration, as well as the poſſibility of a conſtant ſupply of water for ſmall veſſels, without any pumping at all, may in many caſes allow the firſt apparatus to be uſed with advantage, and even render it preferable, eſpecially in elevated ſyphons and great rarefactions, as moſt commonly occur in practice, when it would not only be very difficulty to keep the air-pump tight without great friction, but a quantity of vapour from the heated fluid would be expanded. This increaſes the difficulty of the air-pump, but in the other contrivance it is partly condenſed by the cold water in the air-veſſel trough, which it is obliged to paſs. but would perhaps be very heavy on a ſmall apparatus.

I am with great eſteem, My dear Sir,

Your obedient humble Servant,

A. N. EDELCRANTZ.

II.

Enquiries concerning the Nature of a Metallic Subſtance lately ſold in London as a new Metal, under the title of Palladium.

By RICHARD CHENEVIX, *Esq. F. R. S. and M. R. I. A.* *

ON the 29th of April I learned, by a printed notice † ſent to Mr. Knox, that a ſubſtance, which was announced as a new metal, Announce of a new metal for ſale.

* From the Philoſ. Tranſ. 1803.

† "Palladium, or new ſilver, has theſe properties amongſt others "that ſhew it to be a new noble metal.

"1. It diſſolves in pure ſpirit of nitre, and makes a dark-red "ſolution. 2. Green vitriol throws it down in the ſtate of a re-"gulus from this ſolution, as it always does gold from aqua regia.

"3. It

metal, was to be ſold at Mr. Forſter's, in Gerrard-ſtreet. The mode adopted to make known a diſcovery of ſo much importance, without the name of any creditable perſon except the vender, appeared to me unuſual in ſcience, and was not calculated to inſpire confidence. It was therefore with a view to detect what I conceived to be an impoſition, that I procured a ſpecimen, and undertook ſome experiments to learn it properties and nature.

Its general properties.

I had not proceeded very far, when I perceived that the effects produced by this ſubſtance, upon the various teſts, were ſuch as could not be referred, *in toto*, to any of the known metallic ſubſtances. I immediately returned to Mr. Forſter, and became poſſeſſed of the whole quantity which had been left in his hands for ſale. I could not obtain any information as to its natural ſtate, or any trace that might lead to a probable conjecture.

Laminated ſmall pieces.

The ſubſtance had been worked by art: it had been rolled out in flatting-mills; and was offered for ſale in ſpecimens conſiſting of thin laminæ. The largeſt of them were about three inches in length, and half an inch in breadth, weighing on the average 25 grs. and were ſold for one guinea. The other laminæ were ſmaller, in proportion to the price.

Poliſh, like platina: Elaſticity moderate: Flexibility conſiderable. Specific gravity, differing in the ſpecimens.

Subjected to the ſame treatment as platina, to procure a poliſhed ſurface, palladium aſſumed an appearance ſcarcely to be diſtinguiſhed from that metal. The laminæ were not very elaſtic, but were very flexible, and could be bent ſeveral times in oppoſite directions without breaking. The ſpecific gravity, I found to differ not a little from that which is ſtated in the printed notice, and to vary conſiderably in different ſpecimens. Some pieces of this ſubſtance were as low as 10,972, while others gave 11,482.

" 3. If you evaporate the ſolution, you get a red calx that diſſolves
" in ſpirit of ſalt or other acids. 4. It is thrown down by quick-
" ſilver, and by all the metals but gold, platina, and ſilver. 5. Its
" ſpecific gravity by hammering, was only 11.3; but by flatting,
" as much as 11.8. 6. In a common fire the face of it tarniſhes
" a little, and turns blue, but comes bright again, like other noble
" metals, on being ſtronger heated. 7. The greateſt heat of a
" blackſmith's fire would hardly melt it; 8. But if you touch it,
" while hot, with a ſmall bit of ſulphur, it runs as eaſily as zinc.

" It is ſold only by Mr. Forſter, at No. 26, Gerrard-ſtreet,
" Soho, London; in ſamples of five ſhillings, half a guinea, and
" one guinea each."

The

The effects of galvanic electricity upon palladium, were the same as upon gold and silver. No oxidizement of the substance took place; but oxigen gas was emitted; during the whole time it formed a part of the galvanic circle in action.

Galvanic effects, as gold or silver.

A lamina of this substance being exposed to the blowpipe, the side removed from the immediate action of the flame became blue; but the temperature at which this colour was produced, exceeded that at which steel begins to lose the tinge it had received at a lower heat.

Became blue from heat.

I exposed palladium, in an open vessel, to a greater degree of heat than that which can melt gold. No oxidizement ensued; and, although the metallic slip was extremely thin, no appearance of fusion took place, even at the edges or corners. Upon increasing the fire considerably, I obtained a melted button; but I cannot estimate the degree at which the fusion was effected.

No oxidizement by strong heat. Fusion more difficult than that of gold.

The button, by this treatment, had lost a little of its absolute weight; but its specific gravity had increased from 10,972 to 11,871. It was of a grayish-white. Its hardness was rather superior to that of wrought iron. By the file, it acquired the colour and brilliancy of platina. It was malleable to a great degree. Its fracture was fibrous, and in diverging striæ, which seemed to be composed of crystals; the surface of the button also, when seen through a lens, appeared to be crystallized.

Fused button rather lighter, more dense, harder than iron, malleable, fracture fibrous crystallized.

Palladium very readily combines with sulphur. I exposed a certain quantity of it to a violent heat, without being able to melt it; and, at that elevated temperature, threw some sulphur upon it. It immediately entered into fusion, and remained in that state until the redness of the crucible was hardly visible in the daylight. The increase of weight in the button of the sulphuret, was such as could not indicate with exactness the proportion of sulphur combined with it; and I was so limited in the quantity of palladium I could obtain on any terms, that I thought it prudent to reserve as much as possible for the investigation of more important properties. Sulphuret of palladium is rather whiter than the substance itself, and is extremely brittle.

Combines and flows with sulphur by ignition. Sulphuret whiter and very brittle.

Palladium, melted in a charcoal crucible, and kept in fusion for fifteen minutes, did not acquire any properties different from those which I have already mentioned, in speaking of the

Continued fusion of palladium in charcoal; no change.

effect

effect of heat upon that ſubſtance. Hence we may conclude, that there is not any action between charcoal and palladium.

Alloy. Palladium and gold; gray and leſs ductile.

I put equal parts of palladium and gold into a crucible, for the purpoſe of forming an alloy. The reſult, owing to an accident, did not weigh ſo much as the ſum of the quantities employed; therefore, the proportions in this alloy were uncertain. Its colour was gray; its hardneſs about equal to that of wrought iron. It yielded to the hammer; but was leſs ductile than each metal ſeparate, and broke by repeated percuſſions. Its fracture was coarſe-grained, and bore marks of cryſtallization. Its ſpecific gravity was 11,079.

Palladium and platina; denſer, leſs malleable.

Equal parts of platina and palladium, entered into a fuſion at a heat not much ſuperior to that which was capable of fuſing palladium alone. In colour and hardneſs, this alloy reſembled the former; but it was rather leſs malleable. Its ſpecific gravity, I found to be 15,141.

Palladium and ſilver: whiter.

Palladium, alloyed with an equal weight of ſilver, gave a button of the ſame colour as the preceding alloys. This was harder than ſilver, but not ſo hard as wrought iron; and its poliſhed ſurface was ſomewhat like platina, but whiter. Its ſpecific gravity was 11,290.

Palladium and copper: harder dull colour.

The alloy of equal parts of palladium and copper was a little more yellow than any of the preceeding alloys, and broke more eaſily. It was harder than wrought iron; and, by the file, aſſumed rather a leaden colour. Specific gravity 10,392.

Palladium and lead: more fuſible, gray, cloſe grained, and hard and brittle.

Lead increaſes the fuſibility of palladium. An alloy of theſe metals, but in unknown proportions, was of a gray colour, and its fracture was fine-grained. It was ſuperior to all the former in hardneſs, but was extremely brittle. I found its ſpecific gravity to be 12,000.

Palladium and tin: grayiſh, brittle, compact.

Equal parts of palladium and tin gave a grayiſh button, inferior in hardneſs to wrought iron, and extremely brittle. Its fracture was compact and fine-grained. Specific gravity 8,175.

Palladium and biſmuth; gray, very hard and brittle.

With an equal weight of biſmuth, palladium gave a button ſtill more brittle, and nearly as hard as ſteel. Its colour was gray; but, when reduced to powder, it was much darker. Its ſpecific gravity, I found to be 12,587.

Palladium and [illegible] Palladium [illegible]onic.

Iron, when alloyed with palladium, tends much to diminiſh its ſpecific gravity, and renders it brittle. Arſenic increaſes the fuſibility of palladium, and renders it extremely brittle.

From

From theſe experiments, we may form the following table, ſhewing the difference between the true and the calculated mean of ſpecific gravity in the alloys of palladium.

	Metals.	Proportion.	Specific gravity by calculation *.	Specific gravity by experiment.	Difference.
Palladium alloyed with	Gold -	uncertain.	uncertain.	11,079	uncertain.
	Platina -	equal parts.	17,241	15,141	—2,100
	Silver -	equal parts.	10,996	11,290	+ ,294
	Copper	equal parts.	10,176	10,392	+ ,216
	Lead -	equal parts.	uncertain.	12,000	uncertain.
	Tin -	equal parts.	9,340	8,175	—1,165
	Biſmuth	equal parts.	10,652	12,587	+ 1,935

Compariſon of the ſpecific gravities of theſe alloys with the computed denſities.

I expoſed ten grains of palladium to the action of potaſh, in fuſion during half an hour. The ſubſtance loſt its brilliancy, and diminiſhed two grains and a half in weight; theſe were found in the potaſh.

Palladium fuſed with potaſh loſt weight and brilliancy.

The action of ſoda upon palladium, does not appear to be quite ſo violent.

With ſoda.

Ammonia, allowed to remain for ſome days upon palladium, acquires a ſlight bluiſh tinge, and holds a ſmall portion of oxide of palladium in ſolution. In all theſe caſes, the action of the alkali is promoted by the contact of the atmoſpheric air, the oxigen of which combines the metal, in favour of the affinity the oxide of palladium poſſeſſes towards the alkali.

Ammonia in the humid way determines oxidation, and takes up oxide.

Some of the pieces of palladium were more eaſily acted upon by the acids than others; and, in general, thoſe of the greateſt ſpecific gravity were the leaſt affected. Upon the whole, however, the following ſtatement may be taken as the average of the habitudes of palladium with the acid ſolvents.

* In the ſpecific gravities of the different metals, I have followed the table given in our beſt elementary work, Dr. Thompſon's Syſtem of Chemiſtry.

Sulphuric

Sulphuric acid and palladium. Weak action: beautiful red ſolution.

Sulphuric acid, boiled upon palladium, acquires a beautiful red colour, and diſſolves a portion of the ſubſtance. The action of this acid is not very powerful, and, upon the whole, it cannot be looked upon as a good ſolvent for palladium.

Nitric acid more active; fine red ſolution.

Nitric acid acts with much greater violence upon palladium. It oxidizes the ſubſtance with ſomewhat more difficulty than it can oxidize ſilver; and, by diſſolving the oxide, forms a very beautiful red ſolution. If the nitric acid be impregnated with nitrous gas, its action upon palladium is much more rapid.

Muriatic acid. Red ſolution.

Muriatic acid, by being boiled upon palladium for a conſiderable time, acts upon it, and becomes of a beautiful red.

Nitro-muriatic acid is the true ſolvent.

But the true ſolvent of palladium is nitro-muriatic acid, which attacks it with great violence, and forms a beautiful red ſolution.

Precipitate from acids by alkalis or earths, fine orange partly redisſolved.

From all theſe acid ſolutions of palladium, a precipitate may be produced by the alcalis and earths. Theſe precipitates are, for the moſt part, of a beautiful orange; are partly redisſolved by ſome of the alcalis; and the ſupernatant liquor of the precipitate formed by ammonia is ſometimes of a fine greeniſh blue. Sulphate, nitrate, and muriate, of potaſh, or of ammonia, produce an orange precipitate in the ſalts of palladium, as in thoſe of platina, when not in too dilute ſolution; and the precipitates from the nitrate of palladium are in general of a deeper orange. All the metals, except gold, platina, and ſilver, cauſe very copious precipitates in ſolutions of palladium. Recent muriate of tin produces a dark orange or brown precipitate in neutralized ſalts of palladium, and is an extremely delicate teſt. Green ſulphate of iron precipitates palladium in the metallic ſtate; and, if the experiment ſucceed, the precipitate is about equal in weight to the palladium employed. Pruſſiate of potaſh cauſes an olive-coloured precipitate; and water impregnated with ſulphuretted hidrogen gas, a dark brown one. Fluoric, arſenic, phoſphoric, oxalic, tartaric, citric, and ſome other acids, together with their ſalts, precipitate ſome of the ſolutions of palladium, and form various combinations with this ſubſtance.

Such are the principal characters I have found in palladium, examined as a ſimple metallic body. It does not appear that, in ſtating any of its properties, except its ſpecific gravity, the printed notice has been guilty of miſrepreſentation.

From

From theſe experiments, it would be difficult to ſay of what metal, or of what combination of metals, palladium conſiſts. We could not ſuppoſe gold or platina to be an ingredient in it, as it is in ſome meaſure acted upon by ſulphuric and muriatic acids, and is wholly ſoluble in nitric acid. Silver is excluded, by the effect of muriatic acid upon its ſolutions; as is lead, by that of the ſulphuric. Tin, antimony, biſmuth, or tellurium, would have left an inſoluble reſiduum with nitric acid. No traces could be found of any of the acidifiable metals; and iron was looked for with particular care, but in vain. In a word, the precipitation by the metals, ſeems to exclude all thoſe of eaſier oxidability than mercury; and this we ſhould not ſuppoſe to be preſent, as copper is not in the leaſt whitened, when uſed to precipitate palladium.

Compariſon of this body with the other known metals.

The ſtriking ſimilarity of many of the precipitates of palladium with thoſe of platina, induced me to multiply the comparative experiments; and I conſtantly obſerved contradictory facts. The ſpecific gravity, eaſy fuſibility, combination with ſulphur, precipitation by green ſulphate of iron and by pruſſiate of potaſh, together with other effects, were ſuch as I could not reconcile to the known characters of platina; unleſs I could ſuppoſe that a ſubſtance did exiſt, which could totally change its phyſical and chemical properties, or ſo diſguiſe them as to render them proof againſt the evidence of chemical re-agents.

It reſembles platina in many of its precipitates, &c.

The lighteſt of the metals is tellurium; yet, in order to produce an alloy of the ſpecific gravity of palladium, (ſuppoſing for a moment the real denſity of the alloy equal to the calculated mean), it would require two parts of tellurium and one of platina; and it is highly improbable, that ſo large a proportion of tellurium could exiſt in any maſs, without being detected. We have been told of very extraordinary anomalies in chemical affinities, by Mr. Berthollet; and Mr. Hatchett has made us acquainted with ſome, not leſs extraordinary, in the properties of alloys. Yet I think we ſhall ceaſe to wonder at what has been related by theſe chemiſts, when we learn that palladium is not, as was ſhamefully announced, a new ſimple metal, but an alloy of platina; and that the ſubſtance which can thus maſk the moſt characteriſtic properties of that metal, while it loſes the greater number of its own, is mercury.

It is an alloy of platina and mercury.

I confeſs it was not from an analyſis of palladium that I was firſt led to this reſult; for I had convinced myſelf, by ſyntheſis,

This was ſynthetically diſcovered.

of

of its nature, and had formed the ſubſtance, before I could deviſe any probable method of aſcertaining its component parts.

Admirable diſpoſal of platina and mercury by which they were preſented to each other and united.—Green ſulphate of iron was added to a ſolution of platina; and alſo to a ſolution of mercury. No precipitate took place in either. The compounds were then mixed. Platina and mercury fell down together. The compound was fuſed. It was palladium.

In reflecting upon the various modifications which ſubſtances undergo when in union with each other, and on the variations produced in the laws of affinity by the intervention of new bodies, I was induced to try whether by the affinity of platina with ſome metal eaſily reduced, it might not happen that a reduction of both would take place by green ſulphate of iron, although no ſuch effect were produced upon each metal when ſeparate. The moſt likely to ſucceed, as being moſt eaſily reduced, after gold, platina, and ſilver, was mercury. I poured ſome ſolution of green ſulphate of iron into a ſalt of platina, and alſo into a ſalt of mercury; no precipitation took place I united the two liquors; and a precipitate exactly reſembling that which is formed by green ſulphate of iron in palladium, was inſtantly formed. I collected the precipitate, and expoſed it to a ſtrong heat; and, after repeated trials, obtained a metallic button, not to be diſtinguiſhed from palladium.

It certainly is one of the moſt extraordinary facts reſpecting alloys, that two metals, by their union with each other, ſhould ſo loſe the characteriſtic properties of each individually, that neither of them can be immediately detected by the uſual methods. Nothing but an affinity of the moſt powerful order could produce ſuch effects. But, to place the metals under the moſt favourable circumſtances for that affinity to exert its influence, and to promote their union, is not the reſult of common methods. Among a great number which I have tried, many have failed, and none have been attended with uniform ſucceſs. I have, however, formed palladium by the immediate union of platina and mercury; and, as whatever may place the apparent capricioufneſs of this combination in a conſpicuous point of view is not devoid of intereſt, I ſhall deſcribe the means by which I have attempted to produce it, whether they failed, or were attended with ſucceſs.

Palladium by the immediate union of platina and mercury.

SYNTHETICAL EXPERIMENTS.

Exp. 1. It was not till after repeated trials of the mode juſt mentioned, that I ſucceeded in forming palladium. In many inſtances, I obtained a button completely melted, of the ſpecific gravity of 13, and ſometimes more; not ſo eaſily fuſed by ſulphur as palladium; not ſoluble in nitric acid; and the abſolute

abſolute weight of which exceeded that of the platina originally employed. But, although this ſubſtance was not platina, I could not ſay it was palladium. The moſt ſucceſsful experiment by this method, was attended with the following circumſtances. I diſſolved one hundred grains of platina in nitro-muriatic acid, and then put in two hundred grains of red oxide of mercury, made by nitric acid; but this not being ſufficient to ſaturate the exceſs of acid, I continued to add more, until it ceaſed to be diſſolved. On the other hand, I prepared ſome green ſulphate of iron, and poured it into a long-necked mattraſs. I then poured the mixed ſolution of platina and mercury into the ſolution of green ſulphate of iron, and heated the whole upon a ſand bath. In leſs than half an hour, a copious precipitate was formed; and the inſide of the mattraſs was lined with a thin metallic coat. The liquor was paſſed through a filtre, which I had weighed; and the precipitate, after digeſtion with muriatic acid, was well waſhed and dried. When I had collected as much of this as I could, there remained upon the filtre 12 grains; beſides which, I had collected 264, in all 276. The ſupernatant liquor ſtill contained a portion of mercury, and about eight grains of platina. Therefore, the 276 were compoſed of 92 of platina, and 184 of mercury. From this it appears, that one hundred grains of platina, can determine the precipitation of near two hundred grains of mercury, by green ſulphate of iron; and that, in this proportion, there is a reciprocity of ſaturation. The 264, collected from the filtre, were expoſed to a low red heat, and were reduced to 144. The twelve of the filtre would have given about ſeven; therefore, the whole would have been 151. The ſubſtance was in the form of a fine powder, and had a metallic luſtre. It was then put into a charcoal crucible, and fuſed into a button. This button weighed 123 grains, and with the quantity left on the fibre, would have weighed 135. In this 135, there were 92 of platina; therefore it was compoſed of about two parts of that metal and one of mercury. It was of the ſpecific gravity of 11,2; was wholly ſoluble in nitric acid; was eaſily fuſed by ſulphur; was precipitated by green ſulphate of iron: in a word, it was not to be diſtinguiſhed from palladium.

Experiment 1. The moſt ſucceſsful experiment of forming palladium by the mode firſt mentioned.

It contained about two parts platina and one mercury.

Exper. 2. As another mode of forming palladium in the humid way, I put metallic iron into a mixed ſolution of platina and

Experiment 2. Mixed ſolution of platina and

and

mercury was precipitated by iron. The ſucceſs was leſs complete.

and mercury. Both metals were precipitated; and the precipitate was ſubmitted to the ſame treatment as in the former caſe; but the ſucceſs was not ſo complete. Iron can precipitate either platina or mercury ſeparately; but green ſulphate of iron can perform its function only in favour of the affinity of platina and mercury. Their union is promoted by its action; and the effects are, in all probability, ſimultaneous. The combination of the metals takes place, if I may be allowed the expreſſion, in their naſcent metallic ſtate, and in a fixed proportion of mutual ſaturation. The union of the two metals, therefore, is in the preſent experiment leſs intimate, and the button which reſults from fuſing the precipitate, is of much greater denſity.

Exp. 3. Zinc the precipitant. Incomplete.

Exper. 3. The ſame proceſs was repeated, only uſing zinc inſtead of iron, but the reſult was not more ſatisfactory.

Exp. 4. Mercury added to ſolution of platina. Incomplete.

Exper. 4. I poured ſome mercury into a ſolution of platina, and heated them together for ſome time. A precipitate took place; but, upon fuſing it into a button, I did not find it to be palladium.

Exp. 5. Mixed ſolutions of platina and mercury evaporated and ignited. The mercury flew off.

Exper. 5. I diſſolved the ſame quantities of platina and mercury as in *Exper.* 1, in nitro-muriatic acid, and evaporated thoſe ſolutions together. I then volatilized as much as I could of the mercury, at a red heat. At the end of the operation, I obtained preciſely my original quantity of platina, reduced to the metallic ſtate; but not one particle of the mercury remained along with it.

Exp. 6, 7. Platina and mercury precipitated by phoſphate of ammonia. No ſucceſs.

Exper. 6 and 7. The ſame quantities of platina and mercury, diſſolved in nitro-muriatic acid, were precipitated by phoſphate of ammonia; and the liquor was evaporated. The reſiduum, in a glaſſy ſtate, was expoſed to a violent heat in a charcoal crucible; and I obtained a melted button, which weighed more than the original quantity of platina, and was of the ſpecific gravity of 14,5. On account of the eaſy fuſibility of phoſphuret of platina, I likewiſe tried to combine it directly with mercury, but could not ſucceed.

Phoſphuret of platina.

Exp. 8. Platina and mercury precipitated by ſulphuretted hidrogen. In one inſtance palladium.

Exper. 8. I precipitated a mixed ſolution of platina and mercury, by a current of ſulphuretted hydrogen gas; and reduced the inſoluble powder. After many attempts, in which I obtained buttons of the ſpecific gravity of 14,3 and 14,5, I formed a piece weighing 11 grains, of the ſpecific gravity of 11,5. This

11,5. This laſt was palladium; but I could not aſcertain the exceſs of weight, as a part of the original precipitate had been loſt.

Exp. 9. I mixed a ſolution of muriate of platina with pruſſiate of mercury, and obtained a ſlight precipitate. The liquor was evaporated, and the whole reſiduum expoſed to a violent heat. This experiment did not ſucceed. It was not repeated ſo often as the others; but I have ſome reaſon to think it might be attended with ſucceſs; for I obtained, in one inſtance, a few very minute grains, that were ſoluble in nitric acid.

Exp. 9. Muriate of platina and pruſſiate of mercury. Evaporation and ſtrong heat. Partial ſucceſs.

Exp. 10. I heated ſome purified platina, in the form of a very fine powder, with ten times its weight of mercury, and rubbed them together for a long time. The reſult was, an amalgam of platina. This amalgam, expoſed to a violent heat, loſt all the mercury it had contained; and the original weight of the platina remained without increaſe.

Exp. 10. Amalgam platina and mercury. Heat drove off the mercury.

Exp. 11. The beſt method of forming an amalgam of platina, is that preſcribed by Count Muſſin Puſhkin. I diſſolved a known quantity of platina in nitro-muriatic acid, precipitated by ammonia, and evaporated the liquor. The reſiduum was rubbed for a long time with a great quantity of mercury, and then expoſed to a violent heat. Many operations failed; in ſome, I had a button of the ſpecific gravity of 13,2. In one attempt, I ſucceeded completely: from 30 grs. of platina, treated as above, I obtained a button weighing 43,5, of the ſpecific gravity of 11,736, which had all the properties of palladium.

Exp 11. Count Muſſin Puſhkin's amalgam of platina heated gave palladium in one attempt.

Exp. 12. I fuſed together, in a charcoal crucible, 100 grs. of platina, 200 of cinnabar, 100 of lime, and 400 of calcined borax; and obtained a button, which weighed more than the platina, and was of the ſpecific gravity of 15,7. It was not ſoluble in nitric acid; but combined with ſulphur, at a red heat.

Exp. 12. Platina, cinnabar, lime and borax fuſed.

Exp. 13. In ſome experiments I had made, I found that the furnace in which I formed theſe alloys, was capable of melting platina, without the aſſiſtance of any flux except calcined borax. I therefore urged 100 grs. of platina, at a very ſtrong heat; and, when I judged the fire to have attained its greateſt intenſity, I poured mercury upon the platina, through a long earthen tube that terminated in the crucible,

Exp. 13. Platina fuſed and mercury thrown in. No effect.

and immediately withdrew the apparatus from the fire. No ſenſible union of the metals had taken place; nor had the platina increaſed in weight.

Exp. 14. Mercury in vapour paſſed over ignited platina. No effect.

Exp. 14. I put 100 grains of platina into an earthen tube, and placed the tube horizontally in the above furnace. At one end of it was a retort, containing 2lbs. of mercury. When the tube was at its greateſt heat, the mercury was made to boil; and the entire quantity paſſed over the ſurface of the platina, at that temperature. The experiment laſted one hour and a half; but the metals did not ſeem to have combined.

Exp. 15. Platina wire plunged in mercury and heated by galvaniſm. Little effect.

Exp. 15. Mr. Pepys was ſo obliging as to try the effect of his very powerful galvanic battery, in forming palladium. A piece of platina-wire was plunged into a baſon of mercury, and formed part of a galvanic circle. The wire was nearly in fuſion; but no combination ſeemed to take place. The nature of this experiment did not allow of very accurate weighing; but the fuſed globules of platina did not appear to have acquired the properties that conſtitute palladium.

Remarks. Diſpoſing affinities and aſſimilation.

Such are the experiments by which I attempted to form palladium. They were chiefly founded upon two principles; diſpoſing affinity, and aſſimilation. In the one caſe, I endeavoured to preſent to the metals that compoſe it, a ſubſtance which, on account of its affinity for ſome menſtruum neceſſary for their ſolution, and of their own tendency to combine in the proportions ſtated in *Exp.* 1, might cauſe them to unite in the form of an inſoluble compound. In the other caſe, I hoped to aſſimilate the properties of each, and, by making them ſomething more alike, to place them in the moſt favourable circumſtances for uniting. *Exp.* 1. was founded on the former, and *Exp.* 8 on the latter of theſe principles.

The platina always acquired weight when its properties were thus changed.

Mercury was indiſpenſable.

In many inſtances, when I did not form palladium, I obtained a metallic button which was not platina; and, when I did ſo, it always weighed more than the original quantity of platina employed. In repeating *Experiments* 1, 2, 4, 6, 8, 11, and 12, I ſeldom failed of having ſuch a ſubſtance. No effect of this kind took place in any experiment, when mercury was not uſed along with platina; and the other metals were merely acceſſaries, in promoting their union and precipitation. This is ſufficiently proved, by the uniformity of the reſults in different proceſſes, whether it was palladium or the ſubſtance I now mention which was formed. The chief property which

diſtinguiſhes

diſtinguiſhes the latter ſubſtance from platina, is its denſity. It is not unuſual to obtain it of a ſpecific gravity ſo low as 13; very frequently 15 or 17. In the firſt experiments, I ſuſpected this lightneſs to be owing to ſome air-bubbles; but repeated fuſion, and comparative experiments upon platina, ſoon convinced me of the contrary. The augmentation of weight alſo, which the platina never fails of acquiring, proves that this metal has combined with ſome ponderable ſubſtance; and, in fact, the reſult of theſe operations is, an alloy which is a mean betwixt platina in its pure ſtate and what has been called palladium. It is, conſequently, ſubject to infinite variation. The firſt effects which mercury produces upon platina are, to render it more fuſible, and to diminiſh its ſpecific gravity. The next new property conferred upon it is, the power of uniting with ſulphur; and, laſtly, it becomes ſoluble in nitric acid. It is not however till the ſpecific gravity is below 12, or 12,5 at moſt, that it has acquired this property; and all theſe effects follow the direct order of the increaſe of weight obſervable in the platina.

Mercury firſt renders platina more fuſible and lighter; then renders it combinable with ſulphur; and ſtill more renders it ſoluble in nitric acid, viz. under the ſpecific gravity 12.

It is not very difficult to combine a ſmall quantity of mercury with platina; but, to reſolve the problem completely, and to produce an alloy of theſe metals which ſhall be of ſo low a ſpecific gravity as 11,3, and ſhall be ſoluble in nitric acid, is not ſo eaſily accompliſhed. From the repeated failures which I have experienced in theſe operations, I am much inclined to think that the author of palladium has ſome method of forming it, leſs ſubject to error than any I have mentioned. No doubt that perſeverance would put us in poſſeſſion of his ſecret; but, being prevented by want of leiſure from purſuing theſe reſearches at preſent, I have confined myſelf to eſtabliſhing the fact, and deſcribing the proceſſes which I have employed.

The author of palladium probably has ſome ſteady method of compoſing it.

Having thus acquired a certainty that mercury is a conſtituent part of palladium, I made ſome further experiments upon it, with a view to its analyſis; but they have not been attended with ſo much ſucceſs. It might be expected, from the great number of methods which have failed to form palladium, that many might be found to decompoſe it when formed. But I have found the converſe of ſuch proceſſes as did not ſucceed in producing palladium, to be ineffectual in deſtroying the combination.

Its decompoſition extremely difficult.

ANALYTICAL EXPERIMENTS.

Analytical experiments not ſucceſsful.

Exp. 1, 2, and 3. The converſe of the ſynthetical experiments 1, 2, 3, was made, but without any ſatisfactory reſult.

Exceſs of mercury added to ſolution of palladium.

Exp. 4. The converſe of *Exp.* 4 was made without ſucceſs. I put ſome mercury into a ſolution of palladium, and left them together for ſome time. The precipitate which was formed was palladium, juſt as it had been uſed for the operation.

Palladium ſtrongly and durably heated.

Exp. 5. I expoſed different pieces of palladium to a very violent heat for two hours. In ſome, a diminution of abſolute weight, with an increaſe of ſpecific gravity, took place; in others, neither of theſe effects was produced. The ſpecimens which I had made were chiefly of the latter kind.

Cupellation.

Exp. 6. Cupellation did not afford any ſatisfaction reſpecting the analyſis of palladium; but the heat neceſſary for this purpoſe is ſo great, that I could not place great reliance upon this experiment. It is difficult to detach the button from the cupel with accuracy.

Combuſtion in oxigen,

Exp. 7. I burned ſome palladium in oxigen gas. A white ſmoke aroſe during the combuſtion, and was depoſited upon the ſides of the glaſs jar that contained the gas. But this ſmoke was palladium, and not the mercury ſeparated from it.

by galvaniſm.

Exp. 8. A ſlip of palladium, which Mr. Davy had the goodneſs to expoſe, in my preſence, to the action of the ſtrong galvanic batteries of the Royal Inſtitution, burned with a very vivid light, and a white ſmoke; but no mercury was ſeparated by this operation.

Wonderful differences between mixture and combination.

Affinity not proportioned to the facility of union.

There is not any property of this compound which appears to me ſo wonderful, as that which is manifeſted by theſe experiments. It is a ſtriking proof how unfounded was the opinion of ſome philoſophers, who ſuppoſed that the rapidity of combination was a meaſure of the force of affinity. We do not know of any affinity among chemical bodies which is more powerful than that of platina and mercury appears to be. The obſtacles which muſt be overcome, in order to fix the latter metal, are a proof of this; yet the difficulty of forming this combination to its full extent is extreme. The difference which exiſts between the compound and its elements, when merely mixed, either in ſolution or otherwiſe, cannot be better

better exemplified than by comparing the refult of the 5th fynthetical experiment, with the difficulty of expelling mercury from the compound.

The mercury could not be feparated from the bought palladium; nor from any of the author's real combinations.

I muft here obferve, that all the analytical experiments, and many others, were made, by way of comparifon, upon the palladium I had bought, as well as upon that which I had made. But, although I had myfelf combined the mercury with the platina, and confequently knew it to be in the compound that refulted, I could not fucceed in feparating it. Neither did the fubftance defcribed in a former paragraph, as intermediate between platina and palladium, allow one particle of mercury to efcape from it, by any procefs I have yet been able to devife.

Objections to the word palladium.

The name of palladium conveys to our mind the idea of fomething abfolute, and therefore incapable of gradation. But gradations in alloys are infinite; and the alloy of platina and mercury is fufceptible of infinite variation. Palladium alfo brings to our recollection a contemptible fraud directed againft fcience: the name, therefore, fhould not be admitted. I have called it an alloy; for it differs too much from the ufual idea we have annexed to the word amalgam, but it accurately correfponds with our notions of the name I have adopted.

Thefe new facts are worthy of entire credit.

The facts which I have related in this paper, appear at firft fight to have no fimilar examples in chemiftry; and may not gain immediate aflent from every perfon. The philofopher, indeed, will feel no humiliation in being forced to correct or to extend his knowledge; and will not altogether difbelieve a fact, becaufe he can adduce no parallel inftance, or becaufe it is not in uniſon with his received opinions. Such conduct would be raifing an infurmountable barrier againft the progrefs of fcience; it would be fetting up our own feelings in the place of nature; and attempting to meafure what in itfelf is immeafurable, by the narrow fcale of human comprehenfion.

But let us not confine our view of the facts and principles that have been mentioned, to this fingle inftance. Let us trace them in a more extended circle; and fee whether any thing may be found in nature that can apply to the prefent fubject.

The ftrange diminution of fpecific gravity in this compound muft not prejudice our belief.

The firft prejudice, for fuch I muft call it, againft the prefence of platina in palladium is, the fmall denfity of the alloy. And no doubt it is extraordinary, that a metal the fpecific gravity of which is at leaft 22, (Chabaneau fays 24,) combined with another the fpecific gravity of which is nearly 14,

ſhould produce a maſs of the ſpecific gravity of 10,972; not much more than half of that which calculation would denote, and inferior to either of its elements. In Mr. Hatchett's Paper upon the Alloys of Gold, to which I always refer with pleaſure, we find ſome extraordinary inſtances of anomalies in ſpecific gravity, both in exceſs and diminution upon the calculated mean. His experiments have not been doubted; nor can their accuracy be called in queſtion. The principle of deviation in the true and the calculated mean is therefore admitted. Who then can ſay where this deviation ſhall end, or mark out limits to the operations of nature?

Other ſimilar facts.

Particularly the different ſpecific gravities of gaſiform water or ſteam and a mixture of the gaſes which form it. Theſe are as 7 to 4.

But a no leſs extraordinary inſtance of irregular denſity is daily before our eyes; yet it has not ſo much as attracted our attention. It is true that it is taken from among the gaſes. But, if we ſuppoſe that we have attained accuracy in experiments upon theſe ſubjects, I ſee no reaſon to refuſe their evidence in this inſtance. The denſity of oxygen gas, to that of water is as 1 to 740; and the denſity of hydrogen gas as 1 to 9792. The mean denſity of that proportion of oxygen and hydrogen gaſes which conſtitutes water, is to that of water as 1 to 2098; or, in other words, water is 2098 times heavier than the mean denſity of its elements in the gaſeous ſtate. But water is only 1200 times heavier than ſteam, or water in the ſtate of vapour. Therefore, there is a variation in +, of 898, or nearly half, between the denſity of water and its elements, when both are in the aeriform ſtate. This fact, however, regards bodies only as they remain in the ſame ſtate, whether of ſolidity, liquidity, or fluidity. The anomaly is much greater, if we contemplate them as they paſs from one of theſe ſtates to the other. Yet we muſt not omit the conſideration of ſuch a change, in the inſtance of mercury alloyed with platina; for the former metal, before liquid, becomes ſolid as it enters into the new combination.

The fixation of mercury is a fact analogous to many others, and ought to excite no prejudice.

A ſtronger prejudice will perhaps exiſt againſt the fixation of ſo volatile a ſubſtance as mercury. It is certain that the labours of the alchemiſts have thrown ſome ridicule upon this ſubject, as a philoſophicial purſuit. Men of ſcience have long ſince declined the reſearch; and it is not probable that we are indebted to experiments undertaken in the true ſpirit of philoſophy, for the preſent fixation of mercury. But, the ſame cauſe which induced us to look upon the project as chimerical,

ſhould diſpoſe us to admit it when accompliſhed. Every chemiſt well knows that ſimilar fixations of volatile ſubſtances are not uncommon If an ore containing ſulphur, or arſenic, or antimony, be gently roaſted, a great part of thoſe volatile bodies is driven off; but, if a fuſing heat be ſuddenly applied, the maſs unites in ſuch a manner that a very ſmall ſhare of them eſcapes. Mr. Hatchett has inſtanced an artificial combination of gold and arſenic, from which he could not expel the latter metal, by any degree of heat. Yet arſenic, though leſs fuſible, is not much leſs volatile than mercury. I will alſo add a caſe ſtill more in point; *viz.* the combination of arſenic and platina, which is not to be broken by a fuſing heat.

Inſtances; the volatile bodies ſulphur, arſenic, antimony.

An example of this fact, occurs again in water. The liquefaction or ſolidification of two gaſes to produce water, by a loſs of caloric, never ſhocks our mind, becauſe it is familiar to us. We cannot ſay what loſs of caloric may be ſuſtained by mercury, in order to unite with platina; or how far the preſence of the latter may contribute to expel caloric from the former. We know too, that at any temperature, without the aid of a combuſtible body, to act as a reductive, we have not been able to diſunite the laſt portions of oxygen, from the oxides of iron or of manganeſe. Yet, in the uſual method of reducing a metallic oxide, the oxygen is ſurrounded by a much greater quantity of caloric than is neceſſary to convert it into gas. Every fixation of a volatile ſubſtance is analogous to the preſent queſtion; and they whoſe minds have taken alarm from the novelty of the fact, may thus be familiarized with the neceſſity of admitting it.

Mercury having loſt caloric and combined with platina, may be in the ſame ſituation as oxygen in many oxides; viz. not expellable by mere heat, and perhaps by few affinities.

But, it may be objected, in the inſtances of iron or manganeſe, oxygen is combined with a combuſtible body, and retained in it by a decided and powerful affinity. There is no reaſon to ſuppoſe that ſuch an affinity may not exiſt among metals. We have been forced to acknowledge it, in a few caſes, among the earths; and, from the profound and ſagacious reſearches of Mr. Berthollet, we have learned many new facts, that promiſe us a rapid increaſe of knowledge. I ſhall beg leave to add a few examples, which are taken from that claſs of bodies to which the ſubject of the preſent Paper belongs, and ſhow that the metals obey the general law of mutual attraction.

Metals may combine together by an affinity comparable to that of a metal and oxygen.

(To be concluded in our next.)

III.

Inveſtigation of certain Theorems relating to the Figure of the Earth. By JOHN PLAYFAIR, *F. R. S. Edin. and Profeſſor of Mathematics in the Univerſity of Edinburgh.* *

No complete reſults have yet been had as to the figure and magnitude of the earth.

1. THE obſervations which have been made to determine the magnitude and figure of the earth, have not hitherto led to reſults completely ſatisfactory. They have indeed demonſtrated the compreſſion or oblateneſs of the terreſtrial ſperoid, but they have left an uncertainty as to the quantity of that compreſſion, extending from about the one hundred and ſeventieth, to the three hundred and thirtieth part of the radius of the equator. Between theſe two quantities, the former of which is nearly double of the latter, moſt of the reſults are placed, but in ſuch a manner that thoſe beſt entitled to credit are much nearer to the leaſt extreme than to the greateſt. Sir Iſaac Newton, as is well known, ſuppoſing the earth to be of uniform denſity, aſſigned for the compreſſion at the poles $\frac{1}{230}$, nearly a mean between the two limits juſt mentioned; and it is probable, that, if the compreſſion is leſs than this, it is owing to the increaſe of the denſity towards the centre. Boſcovich, taking a mean from all the meaſures of degrees, ſo as to make the poſitive and negative errors equal, found the difference of the axes of the meridian $= \frac{1}{248}$. By comparing the degrees meaſured by Father Leiſganig in Germany, with eight others that have been meaſured in different latitudes, La Lande finds $\frac{1}{311}$, and, ſuppreſſing the degree in Lapland, which appears to err in exceſs, $\frac{1}{331}$ for the compreſſion. La Place makes it $\frac{1}{321}$; Sejour $\frac{1}{307}$, and, laſtly, Carouge and La Lande $\frac{1}{300}$.

Enumeration.

The reſults of obſerved eccentricity agree with the computations deduced from the length of the ſeconds pendulum.

Theſe reſults, which reduce the eccentricity of the meridians ſo much lower than was once ſuppoſed, agree well with the obſervations of the length of the pendulum made in different latitudes. Were the earth a homogeneous body, Sir Iſaac Newton demonſtrated, that the diminution of gravity under the equator would be $= \frac{1}{230}$, expreſſed by the ſame fraction with the compreſſion at the poles. M. Clairault made afterwards a very important addition to this theorem: for he ſhewed, that, if the earth be not homogeneous, but have a

* Edinburgh Tranſ. Vol. V.

denſity

denſity that varies with any function of the diſtance from the centre, the two fractions, expreſſing the compreſſion at the poles, and the diminution of gravity at the equator, when added together, muſt be of the ſame amount as in the homogeneous ſpheroid, that is, muſt be $= \frac{2}{230}$ or $\frac{1}{115}$. Now, the ſecond pendulum is concluded, from the beſt and moſt recent obſervations, to be longer at the pole than at the equator by $\frac{1}{185}$, and this, taken from $\frac{1}{115}$, leaves $\frac{1}{304}$ for the compreſſion at the poles.

Why have not the obſervations of axes agreed better?

2. But though $\frac{1}{300}$, or ſome fraction not very different from it, ſhould be admitted as the moſt probable value of the compreſſion, or ellipticity, as it is called, of the terreſtrial ſpheroid, it ſtill remains to be explained, why all the obſervations, conſidering the care with which they have been made, do not agree more nearly with this concluſion. Among the cauſes that may be aſſigned for this inconſiſtency, though unavoidable miſtakes, and the imperfection of inſtruments, muſt come in for a part, there can be little doubt that local irregularities in the direction of gravity have had the greateſt ſhare in producing it. Of theſe irregularities, that which ariſes from the attraction of mountains has had its exiſtence proved, and its quantity, in one caſe, aſcertained, by the very accurate obſervations of the preſent aſtronomer-royal at Schehallien in Perthſhire. We may trace the operation of this cauſe in many of the degrees that have been actually meaſured. Thus, in the degree at Turin, when divided into two parts, and each eſtimated ſeparately, that which was to the north of the city, and pointed toward Monte Roſa, the ſecond of the Alps in elevation, and the firſt perhaps in magnitude, was found greater in proportion than that toward the ſouth, the plummet having been attracted by the mountain above mentioned, and the zenith made of conſequence to recede toward the ſouth. There are no doubt ſituations in which the meaſurement of a ſmall arch might, from a ſimilar cauſe, give the radius of curvature of the meridian infinite, or even negative.

Anſ.—Principally becauſe the attractions of mountains have cauſed error in the uſe of the plumb-line.

Another more formidable cauſe of error:

But there is another kind of local irregularity in the direction of gravity, that may alſo have had a great effect in diſturbing the accuracy of the meaſurement of degrees. The irregularity I mean is one ariſing from the unequal denſity of the materials under and not far from the ſurface of the earth; and this cauſe of error is formidable, not only becauſe it may go to a great extent,

extent, but becauſe there is not any viſible mark by which its exiſtence can always be diſtinguiſhed. The difference between the primary and ſecondary ſtrata is probably one of the chief circumſtances on which this inequality depends. The primary ſtrata, eſpecially if we include among them the granite, may often have three times the ſpecific gravity of water, whereas the ſecondary, ſuch as the marly and argillaceous, frequently have not more than twice the ſpecific gravity of that fluid. Suppoſe, then, that a degree is meaſured in a country where the ſtrata are all ſecondary, and happens to terminate near the junction of theſe with the primitive or denſer ſtrata, the line of which junction we ſhall ſuppoſe to lie nearly eaſt and weſt; the ſuperior attraction of the denſer ſtrata muſt draw the plummet toward them, and make the zenith retire in the oppoſite direction; thus diminiſhing the amplitude of the celeſtial arch, and increaſing, of conſequence, the geodetical meaſure aſſigned to a degree. From ſuppoſitions, no way improbable, concerning the denſity and extent of ſuch maſſes of ſtrata, I have found, that the errors, thus produced, may eaſily amount to ten or twelve ſeconds.

that the denſity of the ſtrata near the earth's ſurface muſt be much greater towards ſome regions than others.

This error may be ten or twelve ſeconds.

3. While we continue to draw our concluſions, about the figure of the earth, from the meaſurement of ſingle degrees, there appears to be no way of avoiding, or even of diminiſhing, the effects of theſe errors. But if the arches meaſured are large, and conſiſt each of ſeveral degrees, though there ſhould be the ſame error in determining their celeſtial amplitudes, the effect of that error, with reſpect to the magnitude and figure of the earth, will become inconſiderable, being ſpread out over a greater interval; and it is, therefore, by the compariſon of two ſuch arches that the moſt accurate reſult is likely to be obtained. But, in purſuing this method, ſince the arches meaſured cannot be treated as ſmall quantities, or mere fluxions of the earth's circumference, the calculation muſt be made by rules quite different from thoſe that have been hitherto employed. Theſe new rules are deduced from the following analyſis.

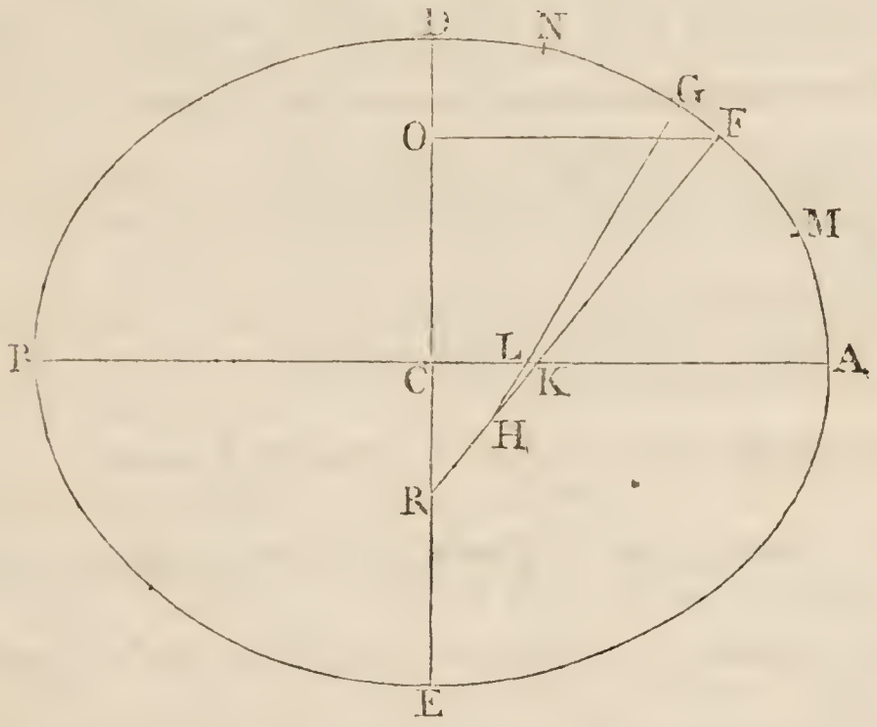
Accurate reſults are therefore only to be had from large arcs;

and theſe require new modes of computation.

4. Let the ellipſis ADBE repreſent a meridian paſſing through the poles D and E, and cutting the equator in A and B. Let C be the centre of the earth, AC, the radius of the equator, $= a$, and DC, half the polar axis, $= b$. Let FG be any very ſmall arch of the meridian, having its

Inveſtigation of formulas for computing the figure of the earth.

its centre of curvature in H; join HF, HG cutting AC in K and L. Let φ be the meaſure of the latitude of F, or the meaſure of the angle AKF, expreſſed, not in degrees and minutes, but in decimals of the radius 1; then the exceſs of the angle ALG above AKF, that is, the angle LHK or GHF will be $= \dot{\varphi}$, and therefore $FG = \dot{\varphi} \times FG$. Alſo, if the elliptic arch $AF = \dot{z}$, $FG = \dot{z} = \dot{\varphi} \times FH$. Inveſtigation of formulas for computing the figure of the earth.



But FH, or the radius of curvature at F, is $= \frac{a^2 b^2}{(a^2 - a^2 \text{ fin } {}^2\varphi + b^2 \text{ fin } {}^2\varphi)^{\frac{3}{2}}} = a^2 b^2 (a^2 - a^2 \text{ fin } {}^2\varphi + b^2 \text{ fin } {}^2\varphi)^{-\frac{3}{2}}$, as is demonſtrated in the conic ſections. Therefore, if c be the compreſſion at the poles, or the exceſs of a above b, $b^2 = a^2 - 2ac + c^2$, or becauſe c is ſmall in compariſon of a, if we reject its powers higher than the firſt, $b^2 = a^2 - 2ac$, and $FH = a^3 (a - 2c) (a^2 - a^2 \text{ fin } {}^2\varphi + a^2 \text{ fin}^2 \varphi - 2ac \text{ fin } {}^2\varphi)^{-\frac{3}{2}} = a^3 (a - 2c) (a^2 - 2ac \text{ fin } {}^2\varphi)^{-\frac{3}{2}}$.

But $(a^2 - 2ac \text{ fin } {}^2\varphi)^{-\frac{3}{2}} = a^{-3} (1 - \frac{2c}{a} \text{ fin } {}^2\varphi)^{-\frac{3}{2}} = a^{-3} (1 + \frac{3c}{a} \text{ fin } {}^2\varphi)$ nearly, rejecting, as before, the terms that involve c^2, &c. Hence $FH = (a - 2c) (1 + \frac{3c}{a} \text{ fin } {}^2\varphi) = a - 2c + 3c \text{ fin } {}^2\varphi$.

Now

Inveſtigation of formulas for computing the figure of the earth.

Now $\dot{z} = \dot{\phi} \times$ FH, therefore $\dot{z} = \dot{\phi}\,(a - 2c + 3c \text{ ſin } {}^2\phi)$ $= (a - 2c)\dot{\phi} + 3c\dot{\phi} \text{ ſin}^2 \dot{\phi}$. But ſin $^2\phi = \frac{1 - \text{coſ } 2\phi}{2}$, therefore $\dot{z} = (a - 2c)\,\dot{\phi} + \frac{3}{2} c\dot{\phi} - \frac{3c\dot{\phi}}{2} \text{coſ } 2\phi$, and taking the fluent $z = \left(a - \frac{c}{2}\right)\phi - \frac{3c}{4} \text{ ſin } 2\phi$. To this value of z no conſtant quantity is to be added, becauſe it vaniſhes when $z = o$.

Therefore an arch of the meridian, extending from the equator to any latitude ϕ, is $= a\phi - \frac{c}{2}\left(\phi + \frac{3}{2} \text{ ſin } 2\phi.\right)$.

5. This theorem is alſo eaſily applied to meaſure an arch of the meridian, intercepted between any two parallels of the equator.

Thus, if MN be any arch of the meridian, ϕ' the latitude of M, one of its extremities, and ϕ'' that of N, its other extremity, we have $\text{AM} = a\phi' - \frac{c}{2}\left(\phi' + \frac{3}{2} \text{ ſin } 2\phi'\right)$, and

$$\text{AN} = a\phi'' - \frac{c}{2}\left(\phi'' + \frac{3}{2} \text{ ſin } 2\phi''\right).$$ therefore the arch

$$\text{MN} = a\,(\phi'' - \phi') - \frac{c}{2}\left((\phi'' - \phi') + \frac{3}{2} \text{ ſin } 2\phi'' - \frac{3}{2} \text{ ſin } 2\phi'\right).$$

6. If, therefore, MN be an arch of ſeveral degrees of the meridian, the length of which is known by actual meaſurement, and alſo the latitude of its two extremities M and N, this laſt formula gives us an equation, in which a and c are the only unknown quantities. In the ſame manner, by the meaſurement of another arch of the meridian, an equation will be found, in which a and c are likewiſe the only unknown quantities. By a compariſon, therefore, of theſe two equations, the values of a and c, that is, of the radius of the equator, and its exceſs above half the polar axis, may be determined.

Thus, if l be the length of an arch meaſured, m the co-efficient of a, and n of c, computed by the laſt formula; and if l' be the length of any other arch, m' the coefficient of a, and n' of c, computed in the ſame manner, we have $ma - nc = l$, and $m'a - n'c = l'$.

Whence $a = \frac{n'l - nl'}{mn' - m'n}$; $c = \frac{m'l - ml'}{mn' - m'n}$ and $\frac{c}{a} = \frac{m'l - ml'}{n'l - nl'}$. It may

may be uſeful, in the numerical calculation, to obſerve alſo that

$$c = \frac{ma - l}{n}.$$

7. The arch of the meridian, which was meaſured in Peru, compared with that meaſured in France, will afford an example of the application of theſe formulas. Application of the formulas to the arc meaſured in Peru and that in France.

The amplitude of the arch meaſured in Peru was 3° 7′ 1″, and its length 176940 toiſes. To reduce this to the level of the ſea, above which it was elevated 1226 toiſes, 66 toiſes muſt be ſubtracted, and again 12 toiſes added to adapt it to the mean temperature of the atmoſphere. Thus corrected it is 176886 toiſes. The arch meaſured begun 36″ north of the equator, and extended to the parallel of 3° 6′ 25″ ſouth; we ſhall ſuppoſe it to have begun under the equator, and to have extended to the parallel of 3° 7′ 1″, a ſuppoſition which can produce no ſenſible error, and will ſomewhat ſimplify the calculation. Thus ϕ, in the preceding formula, is an arch of 3° 7′ 1″ expreſſed in decimals of the radius 1, and ſo we have $m = .0544009$, $n = .1086408$, and $l = 176886$.

Again, the amplitude of the whole arch meaſured in France from Dunkirk to Perpignan is 8° 20′ 2″$\frac{1}{2}$ and its length 475496 toiſes. The northern extremity of this arch is in latitude 51° 2′ 1″, and the ſouthern in 42° 41′ 58″$\frac{1}{2}$. Hence $\phi'' = .8907045$, and $\phi' = .7452459$, and therefore $m' = .1454586$, $n' = .0585735$, $l = 475496$.

Therefore, $a = \frac{n'l - nl'}{mn' - m'n} = 3273325$ toiſes;

$$c = \frac{m'l - ml'}{mn' - m'n} = 10917 \text{ toiſes},$$

and $\frac{c}{a} = \frac{1}{300}$ nearly.

Wherefore alſo the longer axis of the meridian is to its conjugate, or a is to b as 300 to 299.

This proportion agrees well with that which was already pointed out as the moſt probable reſult, from the compariſon of ſingle degrees, and from obſervations of the pendulum. As theſe concluſions are obtained by different methods, they tend greatly to confirm one another.

8. From this too it ſeems highly probable, that the uncertainty which yet remains with reſpect to the true figure of the earth Probability that the figure of the earth will be well determined

by meaſurement of ſome other conſiderable arcs.

earth will be entirely removed by the meaſurement of ſome other conſiderable arches of the meridian. Such an arch will be furniſhed by the ſurvey of Great Britain begun by General Roy, and ſtill continued in a ſtyle of accuracy ſo much ſuperior to any other ſyſtem of geometrical operations that has ever yet been executed. In drawing the concluſions from obſervations made with ſuch exactneſs, it may be neceſſary to employ a more accurate approximation than has been done in the preceding *formulæ*, by retaining the ſecond power of c. The equations to be reſolved will thus become of the ſecond order, but as the unknown quantities can be nearly found by the ſolution of a ſimple equation, the farther approximation to their true values will be accompanied with no difficulty.

Farther approximation to be then employed.

9. Concerning this farther approximation it may be uſeful however to remark, that if c^2 be retained, its coefficient in the formula of § 4 will be $\frac{1}{16a}\left(\varphi + \frac{15}{4}\ \text{fin}\ 4\varphi\right)$;

and therefore in the formula of § 5 it will be

$$\frac{1}{16a}\left(\varphi'' - \varphi' + \frac{15}{4}(\text{fin}\ 4\varphi'' - \text{fin}\ 4\varphi')\right).$$

If then the quantity

$$\frac{1}{16a}\left(\varphi'' - \varphi' + \frac{15}{4}(\text{fin}\ 4\varphi'' - \text{fin}\ 4\varphi')\right),$$

computed for any arch of the meridian, be put $= g$, and the ſame, computed for any other arch, be $= g'$, the equations of § 6 will become,

$$ma - nc + \frac{gc^2}{a} = l, \text{ and}$$

$$m'a - n'c + \frac{g'c^2}{a} = l'.$$

10. Here if we put d for the value of a, as given by the formula $\frac{n'l - nl'}{mn' - m'n}$; and h for the value of c, as given by the formula $\frac{m'l - ml'}{mn' - m'n}$, alſo v for the correction to be made on d, and u for the correction to be made on h, ſo that $a = d + v$, and $c = h + u$, by ſubſtituting theſe values of a and c in the two laſt equations we have $mv - nu + \frac{g(h+u)^2}{d+v} = 0$, and

$$m'v - n'u + \frac{g'(h+u)^2}{d+v} = 0.$$

Hence

Hence, rejecting all the terms that involve v^2, u^2, or uv, we have $dmv - dnu + gh^2 + 2ghv = 0$,
and $dm'v - dn'u + g'h^2 + 2g'hv = 0$.

Farther approximation to be then employed.

Therefore, $v = \frac{(ng' - n'g)\, h^2}{(n'm - nm')\, d + (gn' - g'n)\, 2h}$, alſo

$$u = \frac{g'h^2\,(dm + 2gh) - gh^2\,(dm' + 2g'h)}{dn'\,(dm + 2gh) - dn\,(dm' + 2g'h)}$$

And again, by rejecting thoſe terms that are ſmall in comparison of the reſt, $v = \frac{h^2\,(ng' - n'g)}{d\,(n'm - nm')}$, *and*

$$u = \frac{h^2\,(g'm - gm)}{d\,(n'm - nm')}.$$

Thus v and u are found, and of conſequence $d+v$ and $h+u$, that is a and c, without neglecting any terms that are not of an order leſs than $\frac{c^2}{a}$; and when it is conſidered that $\frac{c^2}{a}$ is leſs than $\frac{1}{22500}$, it will readily be allowed that it is quite unneceſſary to carry the approximation farther.

11. The ſame thing that renders the compariſon of large arches of the meridian uſeful for leſſening the effect of errors ariſing from irregularities in the direction of gravity, makes it ſerve to diminiſh the effect of all the errors of the aſtronomical obſervations at the extremities of the arches, from whatever cauſe they ariſe. They are all diffuſed over a greater interval, and have an effect proportionally leſs in diminiſhing the accuracy of the laſt concluſion.

The errors of obſervation are leſs the larger the arcs.

12. The meaſurement therefore of large arches of the meridian, eſpecially if performed in diſtant countries, is likely to furniſh the beſt *data* for aſcertaining the true figure of the earth; and on this account extenſive and accurate ſurveys, ſuch as that above mentioned, are no leſs intereſting to ſcience, in general, than conducive to national utility. The ſurvey of this iſland, when completed, will furniſh an arch of the meridian, beginning at the ſame parallel where that meaſured in France terminates, and nearly of the ſame extent, ſo that the length of an arch of more than 16°, or almoſt a twentieth of the earth's circumference, will become known. The different portions of this arch compared with one another, or with the

Extenſive ſurveys particularly uſeful.

arch

arch meaſured in Peru, will afford a variety of *data* for determining the true figure of the earth.

Obſervations of perpendiculars to the meridian, and of parallels of latitude.

But ſurveys of the kind now referred to, afford likewiſe other materials from which the ſolution of this great geographical problem may be deduced. Theſe are chiefly of two ſorts, viz. the magnitude of arches, either of the curves perpendicular to the meridian, or of the circles parallel to the equator. Examples of the firſt of theſe have been given by General Roy and Mr. Dalby; the obſervations which follow are directed toward both.

Large perpendiculars are of very difficult meaſurement;

13. With reſpect to the meaſurement of arches perpendicular to the meridian, it may be obſerved, that the directions of gravity at different points of ſuch arches do not interſect one another at all, unleſs the diſtances of thoſe points from the ſaid meridian be very ſmall. On this account the meaſurement of a large arch perpendicular to the meridian would involve in it conſiderable difficulty; to avoid which it is neceſſary that the arch meaſured be but ſmall, or one that does not greatly exceed a ſingle degree. Such meaſurements are of courſe obnoxious to all the errors that ariſe from the deflection of the plumb-line, and cannot therefore furniſh *data* for determining the figure of the earth, equally valuable with thoſe which may be derived from large arches of the meridian. The method of determining the figure of the earth, from degrees of the perpendicular to the meridian, is not however without its advantages, and in certain circumſtances is preferable to any other that proceeds by the meaſurement of arches equally ſmall. This method is twofold; as a degree of the meridian may be compared with a degree of the perpendicular to it in the ſame latitude; or two degrees perpendicular to the meridian, in different latitudes, may be compared with one another. The advantages peculiar to each will appear from the following inveſtigation.

and ſmall ones erroneous from partial gravitation.

To find the axes of a ſpheroid from comparing a deg. of the merid. with one of the perpend.

14. Let it be required to find the axes of an elliptical ſpheroid, from comparing a degree of the meridian in any latitude with a degree of the curve perpendicular to the meridian in the ſame latitude.

Let the ellipſis ADBE (Fig. on page 105) repreſent a meridian, of which a degree is meaſured at F. Let the perpendicular

dicular to the meridian in F meet the leſs axis DE in R. Then R will be the centre of curvature of the circle cutting the meridian at right angles in F; for at any point in that circle indefinitely near to F, the direction of the plumb-line, or of gravity, as it always paſſes through the axis DE, will cut DE in R; it will therefore alſo interſect FR in R, ſo that R is the centre, and FR the radius, of curvature of the perpendicular to the meridian. Let H be the centre of curvature of the meridian itſelf at F: draw FO perpendicular to DE, and let the latitude of F, or the angle OFR = φ. Alſo let AC = a, CD = b, and $a - b = c$, as before.

To find the axes of a ſpheroid from comparing a deg. of the merid. with one of the perpend.

Then from the nature of the ellipſis, FO = $\frac{a^2 \operatorname{cof} \varphi}{\sqrt{a^2 \operatorname{cof}^2 \varphi + b^2 \operatorname{ſin} \varphi^2}}$, and becauſe ſin FRO : 1 :: FO : FR, that is, cof φ : 1 :: FO : FR, FR = $\frac{a^2}{\sqrt{a^2 \operatorname{cof}^2 \varphi + b^2 \operatorname{ſin} \varphi^2}}$; and this, therefore, is the radius of curvature of the ſection of the ſpheroid perpendicular to the meridian at F. But the radius of curvature of the meridian at F, that is FH = $\frac{a^2 b^2}{\sqrt{a^2 \operatorname{cof}^2 \varphi + b^2 \operatorname{ſin}^2 \varphi}}$, therefore

$$\text{FR} : \text{FH} :: \frac{a^2}{(a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{ſin} \varphi^2)^{\frac{1}{2}}} : \frac{a^2 b^2}{(a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{ſin} \varphi^2)^{\frac{3}{2}}},$$

and dividing both by $\frac{a^2}{(a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{ſin} \varphi^2)^{\frac{1}{2}}}$, we have

FR : FH :: a^2 cof $\varphi^2 + b^2$ ſin $\varphi^2 : b^2$,

15. If then D be the length of a degree of the meridian at F and D′ the length of a degree of the circle at right angles to it,

$$\text{D}' : \text{D} :: a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{ſin} \varphi^2 : b^2, \text{ and } \frac{\text{D}'}{\text{D}} = \frac{a^2 \operatorname{cof} \varphi^2 + b^2 \operatorname{ſin} \varphi^2}{b^2} = \frac{a^2}{b^2} \operatorname{cof} \varphi^2 + \operatorname{ſin}^2 \varphi. \text{ Hence}$$

$$\frac{\text{D}'}{\text{D}} - \operatorname{ſin} \varphi^2 = \frac{a^2}{b^2} \operatorname{cof} \varphi^2 \text{ and } \frac{a}{b} = \frac{\sqrt{\frac{\text{D}'}{\text{D}} - \operatorname{ſin} \varphi^2}}{\operatorname{cof} \varphi}.$$

This laſt formula, therefore, gives the ratio of a to b when D, D′ and φ are known.

16. To

To find the axes of a ſpheroid from comparing a deg. of the merid. with one of the perpend.

16. To find a and b themſelves, if $m = 57.2957$, &c. or the number of degrees in the radius, ſo that $mD' = FR = \frac{a^2}{(a^2 \text{ cof } \varphi^2 + b^2 \text{ ſin } \varphi^2)^{\frac{1}{2}}}$, and ſince it has been already ſhewn that $\frac{a^2}{b^2} = \frac{\frac{D'}{D} - \text{ſin } \varphi^2}{\text{cof } \varphi^2}$, or $b^2 = \frac{a^2 \text{ cof } \varphi^2}{\frac{D'}{D} - \text{ſin } \varphi^2}$, there-

fore $mD' = \frac{a^2}{\left(a^2 \text{ cof } \varphi^2 + \frac{a^2 \text{ cof } \varphi^2}{\frac{D'}{D} - \text{ſin } \varphi^2} \times \text{ſin } \varphi^2\right)^{\frac{1}{2}}} =$

$\frac{a}{\text{cof } \varphi \left(1 + \frac{\text{ſin } \varphi^2}{\frac{D'}{D} - \text{ſin } \varphi^2}\right)^{\frac{1}{2}}}$ and $a =$

$mD' \text{ cof } \varphi \sqrt{1 + \frac{\text{ſin } \varphi^2}{\frac{D'}{D} - \text{ſin } \varphi^2}}$.

Now, $1 + \frac{\text{ſin } \varphi^2}{\frac{D'}{D} - \text{ſin } \varphi^2} = \frac{\frac{D'}{D}}{\frac{D'}{D} - \text{ſin } \varphi^2} = \frac{1}{1 - \frac{D}{D'} \text{ſin } \varphi^2}$,

therefore $a = \frac{mD' \text{ cof } \varphi}{\sqrt{1 - \frac{D}{D'} \text{ſin } \varphi^2}}$.

17. This value of a is very convenient for logarithmical calculation; for if $\text{ſin } \varphi \sqrt{\frac{D}{D'}}$ be computed, it will always be leſs than 1, becauſe D' is greater than D, and therefore may be taken for the ſine of an arch ψ, of which arch $\sqrt{1 - \frac{D}{D'} \text{ſin } \varphi^2}$ will of courſe be the coſine, ſo that $a = \frac{mD' \text{ cof } \varphi}{\text{cof } \psi}$.

The ſame method may be uſed for finding $\frac{a}{b}$ from the formula in § 15.

In

In the ſame manner that a has been found, we will obtain To find the axes of a ſpheroid from comparing a deg. of the merid. with one of the perpend.

$$b = \frac{mD' \text{ cof } \varphi^2}{\left(1 - \frac{D}{D'} \text{ ſin } \varphi^2\right)\sqrt{\frac{D'}{D}}}.$$

If we examine theſe formulas in the extreme caſes, viz. when $\varphi = 90°$, and when $\varphi = 0$, we ſhall have in the former caſe $a = \frac{0}{0}$, becauſe cof $\varphi = 0$, and alſo $D' = D$, ſo that $1 - \frac{D}{D'}$ ſin $^2\varphi = 0$. Here therefore a is indefinite, and may be of any magnitude whatever; and it is evident that this is the reſult which the formula ought to give: becauſe at the pole, or when $\varphi = 90°$, the perpendicular to the meridian is itſelf a meridian, and therefore the meaſurement of the two degrees, D and D′, is but the ſame with the meaſurement of one degree.

When $\varphi = 0$, that is at the equator, the circle perpendicular to the meridian is the equator itſelf, and we have then $a = mD'$, a being determined in this caſe by the degree of the equator alone. Here alſo we have $\frac{a}{b} = \sqrt{\frac{D'}{D}}$, which is known to be true.

18. The preceding formulas may be rendered more ſimple, if we aim only at an approximation, which indeed is all that is neceſſary in this inquiry. Since c denotes the compreſſion, or ſince $a - c = b$, and therefore $a^2 - 2ac = b^2$ nearly, conſequently the radius of curvature of the meridian at F, that is

$$mD = \frac{a^2 (a^2 - 2ac)}{(a^2 - 2ac \text{ ſin } \varphi^2)^{\frac{3}{2}}} = \frac{a^3 (a - 2c)}{a^3 \left(1 - \frac{2c}{a} \text{ ſin } \varphi^2\right)^{\frac{3}{2}}} =$$

$(a - 2c)\left(1 - \frac{3c}{a} \text{ ſin } \varphi^2\right)$, or $mD = a - 2c + 3c \text{ ſin } \varphi^2$. In the ſame manner $mD' = a + c \text{ ſin } \varphi^2$. From theſe equations we obtain, rejecting always the higher powers of c,

$$c = \frac{m (D' - D)}{2 \text{ cof } \varphi^2}, \quad a = mD' - \frac{m (D' - D) \text{ ſin } \varphi^2}{2 \text{ col } \varphi^2}; \text{ and}$$

$$\frac{c}{a} = \frac{D' - D}{2D' \text{ cof } \varphi^2}.$$

Theſe formulas may be transformed into others a little more convenient for computation, by putting ſec φ^2 inſtead of

$\frac{1}{\text{col } \varphi^2}$, and $\tan \varphi^2$ inſtead of $\frac{\text{ſin } \varphi^2}{\text{col } \varphi^2}$; we have then,

$$c = \frac{m}{2} (D' - D) \text{ ſec } \varphi^2,$$

$$a = mD' - \frac{m}{2} (D' - D) \tan \varphi^2, \text{ and}$$

$$\frac{c}{a} = \frac{(D' - D)}{2D'} \text{ ſec } \varphi^2.$$

Example of the compreſſion deduced from the meridian and its perpendicular.

19. We may apply theſe formulas to the computation of $\frac{c}{a}$, &c. from the degrees of the meridian and perpendicular, meaſured in the ſouth of England. We find, in one example, (*Phil. Tranſ.* 1795, p. 537), that $D = 60851$ fathoms, $D' = 61182$, the latitude, or φ being $= 50° 41'$. From this $\frac{c}{a} = \frac{D' - D}{2D' \text{ col}^2 \varphi} = \frac{331}{2 \times 61182 \times (\text{col } 50° 41')^2} = \frac{1}{148.4}$, which is nearly the ſame reſult with that deduced in the paſſage juſt referred to. Indeed the ſolution of this problem, contained in the *Trigonometrical Survey*, is quite unexceptionable; and the theorems here offered are not given as containing a more accurate ſolution, but one that is in ſome reſpects more ſimple.

Whether the magnitude of this reſult may prove an irregular figure.

The above compreſſion, if the remarks already made be well founded, is much too great, being more than double of what was obtained from comparing the whole arch of the meridian meaſured in France with the whole of that meaſured in Peru. At the ſame time it is right to obſerve, that all the other compariſons of the degrees of the meridian, with thoſe of the curve perpendicular to it, made from the obſervations in the ſouth of England, agree nearly in giving the ſame oblateneſs to the terreſtrial ſpheroid. For this circumſtance, it is certainly not eaſy to account; the unparalleled accuracy with which the whole of the meaſurement has been conducted, makes it in the higheſt degree improbable that it ariſes from any error; and even if errors were to be admitted, it is not likely that they ſhould all fall on the ſame ſide. The authors of the *Trigonometrical Survey* ſeem willing, therefore, to give up the elliptic figure of the earth, (*Ibid.* p. 527); but before we abandon that very natural and ſimple hypotheſis, it may perhaps be worth while to attend to the following conſiderations.

20. In the part of England, where the meaſures we are now treating of have been taken, the ſtrata are of chalk, and though of great extent, are bordered, on all the ſides that we have acceſs to examine, by ſtrata much denſer and more compact. Toward the weſt the chalk is ſucceeded by limeſtone, and that limeſtone by the primitive ſchiſtus and granite of the weſt of Devonſhire and of Cornwall. On the eaſt we may ſuppoſe that ſomething of the ſame kind takes place, though the ſea prevents us from obſerving it, as the chalky and argillaceous beds extend in this direction to the coaſt, and probably to ſome diſtance beyond it. Now the meridian of Greenwich may be conſidered as dividing the tract of country, occupied by theſe lighter ſtrata, into two parts, in ſuch a manner, that the plummet being carried to a diſtance from it, either eaſt or weſt, approaches to the denſer ſtrata, and is of courſe attracted by them, ſo that the zenith is forced back, as it were, to the meridian of Greenwich, and does not recede from it, in the heavens, at ſo great a rate as the plummet itſelf does, on the earth. Hence the longitudes from this meridian, eſtimated by the arches in the heavens, intercepted between the zenith and the ſaid meridian, will appear leſs than they ought to do; and too much ſpace on the ſurface of the earth will of conſequence be aſſigned as the meaſure of a degree. In this way D′ is made too great; and we may ſuppoſe the circumſtances ſuch that D, on going north or ſouth, is not enlarged in the ſame proportion; hence $\frac{D' - D}{D'}$ will be augmented, and of courſe $\frac{c}{a}$ will be repreſented as too great. This explanation may perhaps appear very hypothetical, and it is certainly propoſed merely as a hypotheſis. It is a hypotheſis, too, that lays claim only to a temporary indulgence, as it is propoſed at the very moment when it may be brought to the trial, and when, by a further continuation of the ſurvey toward the north, it will probably be determined how far the diſtribution of the ſtrata of this country affects the direction of gravity. It will indeed be curious to remark what irregularities take place on advancing into the denſer ſtrata of the north. The limeſtone and ſandſtone ſtrata of the middle part of the iſland will ſucceed to the chalk of the ſouth, the primitive and denſer ſtrata ſtill occupying

Obſervations to ſhew that the irregularity probably ariſes from local circumſtances.

The meridian of Greenwich appears to divide a chalk country, bearing on each ſide upon denſer ſtrata of limeſtone, ſchiſtus, and granite, which, by attracting the plumb-line either way outwards, muſt require a longer meaſure of eaſting or weſting to correſpond with an apparent celeſtial arc.

occupying the weſt, at leaſt at intervals, as in Wales, Cumberland, and Galloway. Further to the north, that is, beyond the Tay, the ſtrata became entirely primitive, moſt of them of the denſeſt kind, and in the interior of the iſland, with a very few exeptions, continue the ſame to its moſt northern extremity. In the ſurvey of Britain, therefore, ſeveral ſituations muſt occur where the plummet, paſſing from lighter to denſer ſtrata, ought to give indications of ſome irregularities in the direction of the gravitating force. It will be ſeen hereafter how far theſe conjectures are verified by experience.

(To be continued.)

IV.

On the Determination of the Length of the Solar Year. By Mr. R. WINTER.

Red Croſs Wharf, London-Bridge,
13th Jan. 1804.

To Mr. NICHOLSON,

SIR,

Determination of the length of the ſolar and ſidual years.

SHOULD you conſider the following communication as deſerving a place in your valuable Journal, by inſerting it you will oblige

Your's, &c.

R. WINTER.

To determine the preciſe length of the Solar Year is an object of primary utility in aſtronomy, as being the baſis for aſcertaining the periods of all the moving bodies in the univerſe.

Various and accurate methods have been given for determining this period; but as the following unites ſimplicity, to the advanced ſtate of ſcience, theſe characters may render it worthy of your conſideration.

Given, the time and place of the ſun in the ecliptic, and the time of its returning to the ſame place; the obliquity of the ecliptic for the preſent time, together with the decreaſe thereof, per year, to find the length of the ſolar year.

According to Ptolemy, the place of the ſun was Sagittarius $21^{\circ}\ 22'\ 50''$ anno. 130, Dec. 13th, $11^{h.}\ 38^{m.}\ 40^{s.}$ (reduced to the

the meridian of London,) its return to that place in 1804 according to Mayers tables improved, is Dec. 13th. $0^{h}\cdot 19^{m}\cdot 36^{s}$ the interval, (allowing for biſſextiles, &c.) is $611416^{D}\cdot 12^{h}\cdot 40^{m}\cdot 56^{s}$ which divided by 1674, the number of years, gives $365^{D}\cdot 5^{h}\cdot 49^{m}\cdot 42^{s}$. Now the obliquity of the ecliptic, as obſerved by Caſſini in 1655, and Flamſtead in 1689, being compared with that obſerved by Hornſby in 1772, Maſkelyne in 1769, Bradley in 1750, and Mayer in 1756, and a mean of their obſervations taken, will give for the decreaſe of the obliquity of the ecliptic 59″ per century, or 35′.″4 per year. According to theſe calculations the obliquity of the ecliptic for 1804 will be about 23° 27′49″; then as the ſun moves over this ſpace in one quarter of a year, ſay as 23° 27′ 49″ is to $\frac{1}{4}$ of the given year, ſo is the decreaſe of the obliquity of the ecliptic per year 35‴4 unto 55″, which taken from $365^{D}\cdot 5^{h}\cdot 49^{m}\cdot 42^{s}$ gives for the length of the ſolar year $365^{D}\cdot 5^{h}\cdot 48^{m}\cdot 47^{s}$ and the time the ſun moves over the proceſſion of the equinoxes is 20 minutes 27 ſeconds, which being added to the ſolar year will give $365^{D}\cdot 6^{h}\cdot 9^{m}\cdot 14^{s}$ for the length of the ſidereal year.

But by aſſuming $\frac{1}{4}$ of the year thus found, and working as before, it will give the length of the year more accurately.

V.

Extracts from a Letter of R. Chenevix, Eſq. F. R. S. from Dreſden, to Charles Hatchett, Eſq. F. R. S. containing Information reſpecting the new Metal contained in Crude Platina; Beet Sugar; the non-exiſtence of Aguſtile as a peculiar Earth; and a Deſcription of a new Furnace for Chemical Operations. Communicated by C. HATCHETT, *Eſq.*

New metal in crude platina.

I SUPPOSE you have heard of the new metal contained in raw platina. Here is what Berthollet has juſt written to me upon the ſubject:

" Des Coſtils diſſolves raw platina in nitro-muriatic acid, and precipitates it by muriate of ammonia at ſeveral times; the firſt portions are yellow, the laſt redder. He reduces the red precipitate, and obtains an alloy. He expoſes this alloy to a current of oxygen, and a blue oxide is volatilized; pure platina remains behind. The blue ſublimate is the oxide of his

new

new metal; on the ſame day Fourcroy and Vauquelin read their paper at the Inſtitute, and mentioned ſimilar experiments."

Palladium aſſerted to be an alloy of the new metal with mercury.

Van Mons writes me word that at Paris they ſay that it is not platina, but this new metal, which with mercury compoſes palladium; but that has very little to ſay to the purpoſe, for the ſingular thing in palladium is, not what regards platina, but what regards mercury, and the fuſibility of the combination. Beſides there muſt have been platina in my palladium, as I took the metal reduced from the red and yellow ſalts indiſcriminately.

Manufacture of beet ſugar by Lampadius.

Lampadius tells me that he has made ſeveral hundred weight of ſugar from the red beet; but the manufactory he eſtabliſhed has failed. It coſt him within two gros per lb. as much as the common ſugar of equal quality, but then he had a reſiduum fit for fermentation, which was clear profit.

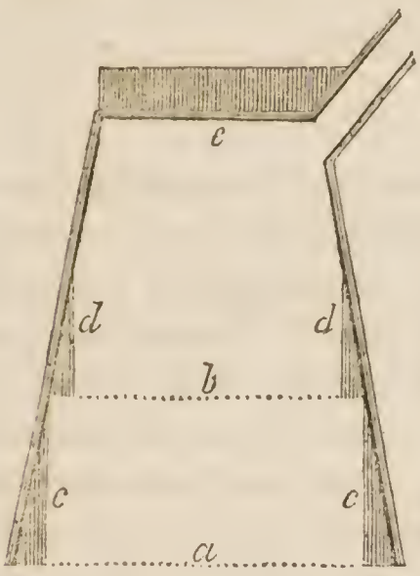
Aguſtite is merely phoſphate of lime.

I had made ſome progreſs in the analyſis of the Saxon beryl, when I received a letter from Berthollet, in which he told me that Vauquelin had found the aguſtite to be nothing more than phoſphate of lime. I continued my experiments however, and obtained the following reſults. Having boiled muriatic acid upon the pulverized maſs which contains the Saxon beryl, I obtained a ſolution of the pretended aguſtite. I precipitated by ammonia, and, upon examining the precipitate, found it to be phoſphate of lime. To have no doubt as to the phoſphoric acid, I treated 1500 grains of this precipitate with half its weight of ſulphuric acid, waſhed the maſs, and evaporated to dryneſs. I re-diſſolved as much as was ſoluble in diſtilled water, and ſaturated the liquor by ammonia. By evaporating once more, I obtained phoſphate of ammonia, which I decompoſed at a ſtrong red heat, and obtained phoſphoric acid, which gave no trace of ſulphuric or any other acid, or of any earth, alkali, or metal. I mixed this with pounded charcoal, and by diſtillation in a ſtrong fire obtained a conſiderable quantity of phoſphorus. Thus then the ſimple earth of Mons. Tromſdorf is phoſphate of lime.

Muriatic ſolution of Saxon beryl precip. by amm. gave phoſ. of lime. This was decompoſed by ſulph. acid; and the purified phoſphoric acid reduced to actual phoſphorus.

I have

I have conſtructed a wind furnace here, which is in ſome reſpects to be preferred to the uſual form. The ſides, inſtead of being perpendicular, are inverted, ſo that the hollow ſpace is pyramidical. At the bottom the opening is thirteen inches ſquare, and at the top but eight. The perpendicular height is 17 inches. This form appears to me to unite the following advantages. 1ſt. A great ſurface is expoſed to the air; which, having an eaſy entrance, ruſhes through the fuel with great rapidity. 2d. The inclined ſides act in ſome meaſure as reverberating ſurfaces: And 3d. The fuel falls of itſelf, and is always in cloſe contact with the crucible, placed near the grate. This I believe to be the principal advantage. The late Dr. Kennedy of Edinburgh, whoſe opinion on this ſubject claims the greateſt weight, found that the ſtrongeſt heat in our common wind furnaces was within two or three inches of the grate. That therefore is the moſt advantageous poſition for the crucible, and ſtill more ſo when we can keep it ſurrounded with fuel. It is inconvenient and dangerous for the crucible to ſtir the fire often to make the fuel fall, and the pyramidical form renders this unneceſſary. It is alſo more eaſy to avoid a ſudden bend in the chimney by the upper part of the furnace advancing as in this conſtruction. *a* is a grate; *c* and *c* are two bricks, which I can let in at pleaſure to diminiſh the capacity; *b* is another grate, which I can place upon the bricks *c* and *c* for ſmaller purpoſes; *d* and *d* are bricks which I can place upon the grate *b* to diminiſh the upper capacity, ſo that in fact I have four different ſizes in the ſame furnace. I have had ſome very ſtrong fires in uſing the whole capacity from the grate *a* to the top *c* without the bricks *c* and *c*; but I am at a loſs for our good Engliſh coaks. The bricks have all been ground down to the ſlope of the furnace and fit in with tolerable accuracy. They are totally independent of the pyramidical form, &c. of the furnace.



Chemical furnace. Fruſtum of a cone, narroweſt above. Its great advantage is that the fuel deſcends without the help of ſtirring.

Account

VI.

Account of an Experiment to imitate the Damaſcus Sword Blades, In a Letter from Mr. JAMES STODART.

To Mr. NICHOLSON,

DEAR SIR,

On ſword blades in general. Quality too often defective.

HAVING lately had an opportunity of examining ſome ſword blades, which appeared to be defective, I was induced to make the following experiment. The ſubject is ſurely of ſome importance, and perhaps never more ſo than at the preſent moment. We hear of ſwords having broken in battle, and we can hardly imagine a more diſtreſſing circumſtance. Thoſe which I have ſeen are certainly in no danger of failing in that way, for on the contrary they are evidently too ſoft, and conſequently cannot form a good cutting edge. I am not acquainted with the proceſs uſed in making ſword blades, but am inclined to ſuſpect that the price allowed is not equal to the labour neceſſary to form a good inſtrument. The following method, which I believe to be nearly the ſame as that practiſed at Damaſcus, but which I ſuſpect would be too difficult and expenſive for general application, may perhaps lead to ſome more ſimple method of accompliſhing the deſired purpoſe. I took ſix ſmall bars of good malleable iron, and the ſame number of ſheer ſteel, and laid them one on another alternately, as if forming a galvanic pile; I then with the aſſiſtance of an expert workman, committed them to a clean forge fire, and with care we ſucceeded in welding them into a ſolid lump. This was forged into a ſtout flat plate, which being heated to whiteneſs, was by means of ſtrong tongs twiſted ſpirally until it formed a cylindrical tube. In this twiſted ſtate it was heated, hammered flat, and again welded, and after being forged into a convenient form and ſubſtance, was doubled throughout its whole length, ſomewhat in the manner of the back of a ſaw. A ſlip of good ſteel was inſerted, and another welding heat taken, which conſolidated the whole maſs. I need not ſay this ſlip of ſteel was intended for our edge. The remaining part of the proceſs was ſimple; it conſiſted only in forging it into the ſhape of the blade we wanted; which on examination proved perfectly ſound in every part. Being eager to witneſs ſome proofs of excellence and beauty which my expectation had

Experiment. Steel and iron bars were welded together.

Forged out.

Twiſted, flattened, again welded and hammered flat.

An edge of ſteel was welded into a back of the compound.

had anticipated, I too haſtily and without due conſideration proceeded to harden it by heating and quenching in water; and had the misfortune to ſee it cracking in ſeven or eight different places. I have no doubt this was occaſioned by the unequal expanſion and ſubſequent contraction of the different parts of the maſs. In my next trial I ſhall guard againſt this accident. Enough however remained found to prove it both good and beautiful; the edge bears the ſevereſt trials at the ſame time that the whole blade has ſufficient tenacity. I have poliſhed a part of it, and by applying a weak acid, produced an appearance, which though by no means equal to the beauty of what is called the Damaſcus water, leaves me little reaſon to doubt of accompliſhing that appearance in my next trial. My intention is to multiply my pieces of metal, to repeat the proceſs of twiſting, and certainly not again to quench in water. I ſhall take the liberty to tranſmit to you an exact account of my next experiment, and if ſucceſsful, to accompany it with a ſample of the metal formed into a blade of ſome kind or another. I am with much reſpect,

It cracked in hardening.

—but promiſes well.

The Damaſcus water.

DEAR SIR,

Your obedient ſervant,

J. STODART.

Strand, Jan. 19th, 1804.

P. S. Why is the appearance produced on Damaſcus ſteel by the application of an acid called the water? Is it not different degrees of oxidation? * and what is the acid beſt fitted to produce this appearance. I had a paper given me ſome ten years ago on this ſubject, by a gentleman whoſe name I do not know. Unfortunately I have miſlaid it.

Inquiries concerning the Damaſcus water upon ſteel.

In addition to what you have publiſhed on the ſubject in your valuable Journal, pray furniſh us with any other facts that may have come to your knowledge ſince that period. The ſubject appears to me to be worthy of philoſophical reſearch, and perhaps of national encouragement.

* I have always ſuppoſed ſteel to be leſs readily ſoluble than pure iron; and that the carbon which is ſeen on the face of the former during the proceſs of damaſking, defends it from the acid, while the fibres of iron are etched by corroſion ſo as to exhibit the peculiar waving lines of this operation. N.

Letter

VII.

Letter from a Correſpondent on the Effects of Thunder on fermenting Liquids; the Chemical Action of Sound, and Tremulous Motions, with other Obſervations.

Newcaſtle, Dec. 16, 1803.

To Mr. NICHOLSON.

SIR,

CONVERSATION is certainly one of the moſt deſirable means for the increaſe and diſſemination of knowledge, whatever deſcription it may be of, and if the following remarks, ariſing from an accidental converſation with a ſcientific friend, be not altogether unworthy of your notice, you may make what uſe of them you think proper; and if thought worthy of being preſented to the public, you are at liberty to make any neceſſary alteration in the ſtile that will render them more worthy of the honour I ſolicit.

Changes produced in beer and in cream by thunder.

It has been obſerved, ſo often indeed, that it has almoſt become a popular remark, that the noiſe of thunder produces a ſurpriſing change on beer, and on cream: the firſt becoming ſour, and the laſt rancid, when they are agitated by this extraordinary ſound. It has alſo been obſerved, I believe, that the effects of thunder on malt liquors may be prevented by laying a cord over the caſk, at each end of which, a ſtone, or other hard and ponderous ſubſtance is ſuſpended and kept in contact with the outſide of the veſſel containing the liquid liable to be affected by this kind of noiſe.

Suppoſed preventative remedy.

The effect ſuppoſed to ariſe from tremulous action.

Now we may be able to form ſome judgment of the manner in which theſe liquids are affected, by attending to the tremulous motion produced on thoſe glaſſes called ſinging and muſical glaſſes, when the ſurface of the firſt is agitated by the breath; and the liquid in the laſt by the vibration produced by the finger drawn along the rim of the glaſs, which varies in its tone in proportion to the quantity of liquid it contains. In the courſe of the tone or ſound, if the glaſs be preſſed with the finger, the ſound either ceaſes or is conſiderably diminiſhed, and upon application of the finger at the commencement of the ſound it will be evidently felt that both the glaſs and the liquid are in a ſtate of agitation, and that this agitation ceaſes or ſtops when the agitated body is touched by another in a quieſcent ſtate.

This

This tremor, ſo communicated, may in my opinion be the commencement of that inſenſible or rather imperceptible agitation neceſſary to the production of the different ſtates of fermentation, which when once began, may be continued, provided the circumſtances of temperature, &c. be equally favourable as at firſt. Now we perceive that this motion is produced by ſound;* and a ſonorous body loſes this vibration on being touched, and the ſound itſelf of courſe ceaſes. In like manner the inteſtine motion ariſing from the vibration occaſioned by the noiſe of thunder, being prevented from taking place by the contact of the ſuſpended ſtones with the ſides of the caſk, the enſuing fermentation cannot commence, and the beer is preſerved from running into the acetous fermentation.

Which may produce the commencement of the fermentative agitation.

This inſenſible motion is alſo neceſſary to aſſiſt the abſorbtion of oxygen, without which neither beer can grow ſour, nor can cream become rancid; for violent agitation will not produce the like effect even though accompanied or aſſiſted with the neceſſary increaſe of temperature.

It was alſo obſerved by the gentleman with whom I was converſing on this ſubject, that butter by being over much waſhed or wrought in water loſes conſiderably of its yellow colour, and acquires in a great meaſure the paleneſs and conſiſtence of tallow. It might be worth while to make ſome experiments in this way, how far animal oils or fat are capable of decompoſing atmoſpheric air or water. The circumſtances neceſſary to determine the mutual change on each, are agitation, and a frequent change of ſurface. The abſorption of oxygen would take place in both caſes, the reſiduum of atmoſpheric air being azotic gas, and that of water hydrogen gas. I had found by experiments, and that ſome time before I ſaw the circumſtance publicly noticed, I may ſay, even before the new theory of chemiſtry was promulgated, that the union between mercury and the fat with which it was combined in the compoſition of the ointment, became much more intimate and perfect on being kept for ſome time, and that the rancidity, which I thought aroſe from the developement of ſome peculiar acid, was prevented from becoming ſenſible by that acid being abſorbed by the metal. From this circumſtance, and from this ſuppoſition, I was led contrary to the directions and *commands*

Decompoſition of water or air by oils.

Rancid oils combine better with mercury than freſh.

* I mean among other cauſes producing it.

of

of the Royal College of Phyſicians, to uſe a little rancid fat or oil, in order to facilitate my labour and favour my indolence, and I was agreeably ſurpriſed at my ſucceſs. The College order the pureſt and ſweeteſt fat to be uſed; nevertheleſs the utmoſt diligence and care in rubbing the mercury with it does not produce ſuch an accurate combination at firſt as is accompliſhed, after being ſet by for a week or two, and this my be made evident by comparing two ſpecimens of ointment made with equal care and diligence, the one being quite recently finiſhed, and the other having afterwards been kept undiſturbed for two or three weeks.—I merely offer theſe hints for the conſideration of wiſer heads than mine, and ſhould they with this view be honoured with a place in your valuable repoſitory, your indulgence will be moſt reſpectfully acknowledged by

Sir,

Yours, &c.

NORTHUMBRIENSIS.

VIII.

Improvement of the Electrical Machine, chiefly with reſpect to the Rubbers; by Mr. WOLFF, *of Hanover* *.

Deſcription of the machine.

The plate.

MY electrical apparatus is a plate machine, conſtructed on the plan of that deſcribed by Van-Marum †, with a few ſlight alterations, which are principally in the rubbers. The plate is eighteen inches in diameter, and the piece of wood, which receives the ſcrew that faſtens the plate to the axis, is an inch and half in diameter. The rubbers (at the extremities of which, toward the axis, are ſticks of black ſealing-wax, rounded at each end, and overhanging or projecting on three ſides) are in contact with the glaſs for the length of $5\frac{3}{8}$ inches. Conſequently the diſtance from the circular or central piece of wood to the rubbers is $2\frac{1}{8}$ inches, to which extent the plate

* From Gilbert's *Annalen der Phyſick*, 1802, No. 13. p. 601.

† *Obſervations ſur la Phyſique*, Vol. 38. p. 437. or Philoſophical Journal, 4to. Series, No. 2.

is coated on both ſides with a ſufficient thickneſs of varniſh *. The bow of the conductor, which conſiſts of a ball ſix inches in diameter, to which is added a moveable tube with ſeveral fitting pieces that terminate in balls of different ſizes, had at firſt, at each of its extremities, as in Van-Marum's machine, a braſs cylinder three inches long and one inch thick, with an hemiſphere at each end. But as conductors of this conſtruction are liable to diſcharge themſelves, on account of the ſmall diameter of their hemiſpheres, even before they have received their full charge, I have preferred putting one braſs ball three inches in diameter on the upper end of the ſuperior conductor; and another on the lower end of the inferior conductor. Theſe balls likewiſe ſerve the purpoſe of collecting the fluid excited at the extreme parts; for the balls, both above and below, project conſiderably beyond the thickneſs of the plate, and prevent the eſcape of the fluid as much as is poſſible, particularly in a machine leſs high in proportion than that of Van-Marum. The conductor is only $4\frac{1}{2}$ inches diſtant from the table, conſequently only half the diſtance of Van-Marum's; yet the fluid does not eſcape, till the conductor is overcharged. The eſcape of the fluid is prevented likewiſe by an inſulating ſquare of glaſs 42 inches, made rough and coated with copal varniſh, which is placed on the table beneath the lower conductor, its three ſhort feet being let into the table. This ſquare of glaſs may be uſed on ſeveral occaſions as an inſulating ſtand.

The conductor deſcribed.

The eſcape of electricity to a glaſs plate placed on the table beneath the conductor.

The four rubbers are made of dry walnut wood ſoaked in amber varniſh, and are $5\frac{3}{8}$ inches long, $1\frac{1}{2}$ broad, and a little more than $\frac{1}{4}$ of an inch thick. The metallic plate that communicates with the leather covered with amalgam, is only an $1\frac{1}{2}$ inch broad, and is fixed externally to the centre of the piece of wood. The rubbers are preſſed toward the glaſs by means of a ſpring. They are covered with a piece of thick woollen, upon which is a piece of fine neat's leather. After the leather is faſtened to the wood, it is wetted, and preſſed between

The rubbers deſcribed.

* This varniſh is compoſed of copal, two ounces; gum ſandarac, one ounce; white amber, one ounce; olibanum, half an ounce; powdered and diſſolved in one pound of alcohol by digeſtion in a glaſs matras. When the ſolution is perfectly cold, it is paſſed through the filter.

Amber varniſh for coating the plate.

two

two boards, where it is kept till it is again dry. Thus it is rendered very flat, and its edge very ſharp, and all its parts will apply to the ſurface of the glaſs. This piece of leather is covered with another a little broader, the rough ſurface of which is towards the glaſs, and its lower edge on the ſide towards which the plate moves; and its upper edge on the other ſide from which the plate moves, being likewiſe very ſharp. The piece of ſilk is applied with accuracy to this leather. Before it is faſtened on, it is heated, and beſmeared firſt with butter of cacao, then with a large quantity of Kienmayer's amalgam *; and after it is faſtened on, it is compreſſed in conjunction with the wood, or preſſed ſtrongly againſt the machine. The leather is next covered with amber varniſh, amalgam is ſpread over this, and after the varniſh is dry, it is ſmoothed with a burniſher. This is repeated ſeveral times. The whole being very dry, and the rubber being preſſed ſo as to touch the glaſs in all points, the leather coated with amalgam is covered with a piece of fine white paper, as long as the leather, and half an inch broader, ſo as to cover the ſeam that faſtens the ſilk to the leather, and the paper is faſtened to the wood above or below, accordingly as it is on the aſcending or deſcending ſide of the plate.

The rubber covered with fine white paper.

What led to this.

Its advantages.

Dry paper is known to be capable of acquiring a high ſtate of electricity, which induced me to try this ſubſtance as an immediate rubber. The following are the advantages, that by my experiments, repeated and varied in a great number of ways, I have found paper employed as a rubber to poſſeſs over every other known ſubſtance.

The glaſs not rendered dull,

1. The glaſs is not rendered dull by the friction, as happens at length, and by frequent uſing, when it is in immediate contact with the amalgam.

does not become ſtreaky,

2. By the immediate contact of the amalgam, the glaſs frequently contracts ſtreaks here and there, that occaſion a circulation of the fluid. This cannot take place in the conſtruction I propoſe.

and neither it nor the ſilk is ſoiled. Cleanneſs of the whole machine advantageous.

3. Neither the glaſs nor the ſilk can be ſoiled. It is well known, that the cleanneſs of the glaſs, as well as of the rubber and the whole machine in general, is of importance in

* I add to this amalgam as much ſilver, as the mercury can diſſolve in conjunction with the zinc.

producing an intenſe degree of electricity. It is true, that it has been propoſed to apply the amalgam to the glaſs inſtead of the rubbers; but the greater effect, that ſeems to be produced by this laſt method, is only apparent, and conſiſts entirely in the circulation of the fluid on the glaſs, while far from exciting or accumulating more of the fluid, this proceſs and the circulation diſperſe it. Rubbing the plate with amalgam inſtead of the rubbers a bad method.

4. The amalgam on the leather does not require to be frequently renewed. The duſt of the amalgam, that is depoſited on the edges of the paper, is injurious only when accumulated there in ſufficient quantity to be conveyed to the glaſs, from which however it may eaſily be removed. The amalgam does not require frequent renewing.

5. The return and paſſage of ſparks to the rubbers are rendered more difficult, as the paper ſufficiently covers the borders of the rubbers, that are turned toward the axis. The return and paſſage of ſparks to the rubbers more difficult.

6. In my conſtruction the rubbers may be larger than in the uſual way, and in reality they are larger in proportion in my machine than in Van-Marum's. No ſpark paſſes the axis, unleſs the air be very damp; for the fluid, in caſe of a ſtrong accumulation, flies in preference to the oppoſite rubber over a quarter of the periphery of the glaſs. I am perſuaded, that, by adopting my conſtruction, the rubbers of a plate of 32 inches, ſuch as Van-Marum's is, may be eleven inches inſtead of nine, in which caſe there would ſtill be two inches for the diameter of the piece of wood that faſtens the plate to the axis, and three inches for the diſtance from this piece to the rubbers; which I think would be ſufficient in theſe circumſtances; and the friction being on a larger ſurface of the plate, the effect muſt naturally be much greater. I ſhall try this alteration of the rubbers on large plates of Bohemian glaſs, as well as on Engliſh cylinders of 18 inches diameter, and 21 inches long. The reſult I have already obtained with a ſmall cylinder gives me reaſon to hope much more complete ſucceſs with a large one. The rubbers may be longer. Sparks do not paſs to the axis. Farther trials intended.

7. With my rubbers the friction may be rendered much greater, than with thoſe the amalgam of which is in immediate contact with the glaſs, and ſoils it; beſides, the plate turns with an uniform friction. The friction may be made much greater, and is uniform.

8. The activity of the machine is extraordinarily increaſed by this conſtruction. The greater freedom with which the plate The activity of the machine extraordinarily increaſed, and why.

plate moves, even under a greater preſſure, and he paper's preventing the glaſs from being ſoiled, would be ſufficient to produce this effect; even if the greater preſſure alone did not occaſion a more powerful effect than can be obtained from common machines.

IX.

On the Cultivation of the Sunflower, and its Advantages *.

The ſunflower recommended to cultivators.

THE ſunflower, *helianthus annuus L.* is ſaid to have been brought to Europe from Peru. There are two ſpecies, one annual, the other perennial: of theſe the latter is only ornamental, but the annual is of ſuch uſe in agriculture, that it deſerves to be made known to our readers.

Seeds, have two varieties.

The ſeeds are white, gray, or blackiſh; but this difference of colour is entirely accidental, as ſeeds of either colour produce the others reciprocally. There are two varieties, however, that appear to me permanent, the ſunflower with a ſingle ſtem, and that with a branchy ſtem, which is leſs common.

Method of cultivation.

The ſeed ſhould be ſowed in ſpring, when nothing is to be apprehended from the froſt, that would deſtroy the young plants. The ground ſhould be well broken and manured, if you would have an abundant crop. You may ſow broadcaſt, and afterward thin the plants: but it is better to dibble the ſeed, placing two in a hole, the holes a foot diſtant, and the rows two feet aſunder. If both ſeeds germinate, the weaker plant ſhould be pulled up. The plants ſhould be weeded, and the earth dug up between the rows at a proper time. The height of the plant will be from ſix to nine feet: the ſtalks are large, ſome of them being ſeven or eight inches in circumference near the ground. It flowers in July or Auguſt, and the ſeeds are ripe in Autumn, at the ſame time with Indian corn. Rainy ſeaſons deſtroy many of the plants: the foot of the ſtalk rots in the ground, the leaves ſuddenly dry up, the ſtalk breaks of at the root, and the plant dies. A few ſunſhiny days ſtop the progreſs of this diſaſter.

Rainy ſeaſons bad.

* *La Decade Philoſophique*, No. 26. June, 1803. p. 507.

The

The leaves of the ſunflower furniſh abundance of agreeable fodder for cattle: they are gathered in ſucceſſion without any perceptible injury to the plant; and after this crop of excellent fodder, you may expect another of ſeeds, that is very abundant. Some ſtalks afford not leſs than ten thouſand. The beſt mode of gathering them is to cut the flower ſtalks, and, as the calyx is very thick, to hang them up in an airy place, that they may dry ſpeedily.

The leaves are excellent fodder.

Mode of gathering the ſeed.

When they are in flower bees flock to them from all quarters to gather honey.

Bees fond of the flowers.

The ſeed is rather farinaceous than oily, which they who have attempted to expreſs oil from them did not conſider. It is true, that oil may be extracted from them, but in quantities too ſmall to make it worth while to cultivate the plant for this purpoſe.

The ſeed affords oil, but in ſmall quantities.

But if the ſeeds of the ſubſtance be incapable of affording oil with advantage, they are valuable for feeding animals commonly kept in the country. They are perfectly well ſuited to ſheep, pigs, and other animals; but they produce a more ſtriking effect on poultry. For theſe no food is more profitable, or occaſions them to lay more eggss.

Good food for ſheep, pigs, &c.

particularly for poultry; promoting their laying.

The dry ſtalks burn well, and afford very good aſhes for lye, becauſe they contain a great deal of alkali. In ſhort, from the eaſe with which it is cultivated, the abundance of its produce, and the change it makes in crops, the ſunflower may be conſidered as a new ſource of wealth to the farmer.

The ſtalks burn well, and afford much alkali.

X.

Chemical Examination of a new Vegetable Salt, and of a new Acid diſcovered in it: by KLAPROTH *.

THE vegetable production, which forms the ſubject of the preſent examination, affords a freſh proof, that hot climates have not only a great influence on the elaboration and particular modification of a great number of vegetable ſubſtances, but that they cauſe productions to be formed, the leaſt veſtige of which is not to be found in the ſame plants in the northern regions.

Hot climates modify the vegetable products of cold, and create new ones.

* Scherer's *Allegemeines Journal der Chemie*, 1803. No. 55. p. 1.

A ſaline maſs exuded from the trunk of a mulberry tree in Sicily.

This production conſiſts of a ſaline maſs, excluded from the trunk of the white mulberry, *morus alba L.* which was obſerved and gathered by Mr. Thompſon, in the botanical garden at Palermo.

External characters.

The ſaline maſs in its native ſalt, as found on the ſurface of the bark, has the appearance of a coating in little gramulous drops of a yellowiſh and blackiſh brown. The ſubſtance of the bark is equally penetrated with this ſalt.

Taſte.

The firſt property by which it is diſtinguiſhed is its taſte, which comes neareſt to that of the ſuccinic acid.

Effect of heat.

On burning coals this ſalt ſwells up ſlightly, emiting a vapour ſcarcely perceptible to the eye, but irritating the organ of ſmell, and leaves a ſlight earthy reſiduum.

Lixiviation affords a light, pale ſalt, in-radiating needles; not deliqueſcent.

To ſeparate the ſalt, ſix hundred grains of bark loaded with it were cut off, and lixiviated with a ſufficient quantity of water. The lixivium, which was of a browniſh red colour was filtered, and evaporated for cryſtallization; when three hundred and twenty grains of a light ſalt were obtained, reſembling in colour a pale wood, and compoſed of ſhort needles united in radii, and not attracting the moiſture of the air.

Little ſoluble in water, though ſoon cryſtallizable.

Though theſe cryſtals do not form till the lixivium is greatly condenſed by evaporation, the ſalt, nevertheleſs, belongs to the claſs of thoſe that are little ſoluble; for 1000 parts of water diſſolve only 35 parts with heat, and 15 parts cold.

Contains no ſulphuric acid.

The ſolution of this ſalt was not rendered turbid by the acetite or the water of barytes, which proves, that it contains no ſulphuric acid.

Lime precipitated from it.

The alcalinule carbonates precipitate from it, an earth of a brown wood colour, which by a ſlight heat is calcined to whiteneſs. This earth diſſolves with effervescence in nitric acid, and was precipitated from this ſolution in the form of ſulphate of lime by means of ſulphuric acid; and, in that of oxalate of lime by oxalic acid.

Precipitates acetite of lead, and nitrate of ſilver and mercury.

The ſolution of the ſalt readily precipitates acetite of lead, and the precipitate formed is reduced on burning coals. The nitrate of ſilver was precipitated by it in a pale brown, light, and ſhining ſcales, and the nitrate of mercury in whitiſh flocks.

Hence it contains lime, with a peculiar vegetable acid, and extractive matter.

Theſe experiments indicate, that the ſalt in queſtion is a neutral ſalt, compoſed of lime and a peculiar vegetable acid. This acid, however, was combined with an extractive matter, which rendered the precipitates, that otherwiſe would have been white, either of a deep or pale wood colour.

Fifty

Fifty grains of the calcareous ſalt were heated to incandeſcence, in a retort communicating with the pneumatico-chemical mercurial apparatus, and twelve cubic inches of hidrogen gas mingled with carbonic gas were obtained; that burnt with a ſtrong flame. The bulb of the neck of the retort contained an acid liquor, on which ſwam a fluid brown oil, both together weighing ſeven or eight grains. The reſiduum in the retort conſiſted of a ſpongy conglobulated maſs of a pale brown colour, interſperſed with coally particles, and diſſolved in nitric acid with effervescence. This ſolution, being filtered, was precipitated with carbonate of ammonia; when 21 grains of carbonate of lime were ſeparated. The coally matter that remained on the filter was very light, weighed four grains and half, and burned on a teſt, leaving a reſiduum of a quarter of a grain of calcareous earth.

By ſtrong heat it gave hidrogen and carbonic gaſes,—

an acid liquor, and a fluid brown oil,

having a ſpungy reſiduum, containing lime, and a carbonaceous matter.

Another portion of the calcareous ſalt was precipitated by carbonate of ammonia, and the liquid remaining was evaporated by a gentle heat to cryſtallize it, when it furniſhed a ſalt in long and ſlender priſms.

By carbonate of ammonia, a ſalt in long ſlender priſms was obtained.

The liquor left, after the cryſtals were ſeparated, was examined by ſeveral metallic re-agents. The ſolutions of ſilver, mercury, copper, iron, cobalt, and uranite in nitric acid, and thoſe of lead and iron in acetic acid, were powerfully precipitated by it. The precipitate of copper was of the colour of verdigris; that of cobalt of a pale reddiſh colour; that of uranite of a yellowiſh colour; that of iron of a dull brown; and thoſe of ſilver, mercury, and lead, of a bright brown wood colour.

The remaining liquor precipitated various metallic ſolutions.

The ſame liquor was rendered ſlightly turbid after a time by the water and acetite of barytes, the muriate of tin and gold, and the nitrate of nickel; but theſe precipitations might be the effect of the extractive matter, that adhered to the acid, rather than of a combination of this with the metallic oxides.

And the re-agents, perhaps owing to the extractive matter, rendered it ſlightly turbid.

I ſhall proceed to the experiments I made with a view of obtaining the acid of the calcareous ſalt pure.

Experiments to obtain the acid pure.

Forty-five grains of the precipitate obtained from the decompoſition of the calcareous ſalt, by the acetite of lead, were mixed with a ſcruple of ſulphuric acid diluted by a drachm of water. The ſulphate of lead was ſeparated, and the liquor evaporated. It afforded by cryſtallization thirty-four grains of acid ſalt in fine needles of the colour of pale wood.

The precipitate with acetite of lead mixed with ſulphuric acid.

Decompoſed by ſulphuric acid.

Decompoſed by Sulphuric acid.

In the ſame manner thirty grains of the dry calcareous ſalt were decompoſed by twelve grains of ſulphuric acid, properly diluted. The ſulphate of lime being ſeparated, the ſame acid ſalt was obtained.

Has the taſte of ſuccinic acid; is not deliqueſcent, diſſolves readily in alcohol and does not precipitate metallic ſolutions.

The taſte of ſuccinic acid is ſtill more marked in the acid itſelf. The acid ſalt remains dry in the air; it is eaſily ſoluble in alcohol as well as in water. It does not precipitate the metallic ſolutions like its ſalt.

In the retort an acid liquor came over; priſmatic, colourleſs, tranſparent cryſtals ſublimed; and a coal remained.

Twenty grains of the acid ſalt were ſlightly heated in a ſmall glaſs retort. Firſt, a couple of drops of an acid liquor came over, the taſte of which was perfectly analogous to that of the concrete acid. Next a concrete ſalt aroſe, that adhered flat againſt the top and part of the neck of the retort, in the form of priſmatic cryſtals, colourleſs and tranſparent. A coally reſiduum remained in the retort.

The acid adheres ſtrongly to the lime.

A ſimilar ſaline ſublimation was not obſervable with the calcareous ſalt: we muſt therefore conclude, that the acid adheres ſtrongly to the lime, and cannot be ſeparated from it by heat, without being decompoſed.

The acid waſhed out and cryſtallized.

To ſeparate the ſublimed ſalt from the coally reſiduum, the whole contents of the retort were diſſolved, and the liquor filtered. The ſolution was perfectly clear, and by ſpontaneous evaporation depoſited the acid ſalt in colourleſs cryſtals.

Sublimation the beſt mode of obtaining it pure.

From this it appears, that a gentle ſublimation is the beſt mode of obtaining the pure acid ſalt, and freeing it from the extractive matter, to which it adheres too ſtrongly to be ſeparated from it in the moiſt way.

Perhaps may be obtained from mulberry trees in this climate.

The ſmall quantity of the calcareous ſalt I had left, did not allow me to carry the examination as far as I could have wiſhed, in order to determine the ſpecific characters of the acid. It is poſſible, that the mulberry trees in this climate may likewiſe furniſh the ſame ſalt, and I ſhall proceed to inquire into this without delay.

A new acid, the *moroxylic*; the ſalt *moroxylate of lime*.

The experiments related, however, ſeem ſufficiently to eſtabliſh the point of the acid obtained, being a new vegetable acid, which comes neareſt to the ſuccinic, both in its taſte and other qualities. We may therefore proviſionally give it the name of *moroxylic acid*, and the calcareous ſalt containing it that of *moroxylate of lime*.

XI. *On*

XI.

On Mr. WOOLF's *Invention for equalizing the Action of a Crank, in contradiſtinction to the Effect of the Fly, in producing an uniform Effect againſt a variable Reſiſtance. In a Letter from Mr.* T. JONES.

To Mr. NICHOLSON,

SIR,

I BEG leave to ſecond Mr. Farcy's requeſt for an explanation of the ſubſtitute for the fly and equalization of the action of the ſteam engine, in producing a rotative motion, given in your valuable work for November laſt: or, I ſhall eſteem it a favour if you can direct me where I can ſee it at work *. It appears to me, that the inventor has loſt ſight of the value of that organ which he intends to ſuperſede, and that he has not applied it in caſes, where a machine is required to produce an uniform effect on a variable reſiſtance, in which caſe the mere equaliſation of the action of the power on a revolving crank would have no value at all. Inquiry reſpecting Mr. Woolf's ſubſtitute for a fly.

The beſt attempt that I have ſeen to unite theſe two important points, is in a ſteam engine erected by Mr. Hornblower, at Meſſrs. Meux and Co's. brewery, where the action of the power on the pin of the crank, may have been in the ratio of the ſines of each reſpective arc in its revolution round the arbor of the fly, I ſay, "*may have been,*" and I wonder it was not, ſince that mode of communication would have ſo eaſily admitted it,—theoretically at leaſt: then indeed, a very light fly would ſerve to turn the crank paſt the upper and nether part or points of the circle; but it could not have anſwered in regulating the unequal reſiſtance which is found even in grinding malt, or pump works: and in my humble judgment, the fly muſt for ever keep its place and importance with every judicious mechanic. Another attempt at the ſame effect.

I am, in much bodily indiſpoſition,

SIR,

Your humble ſervant,

T. JONES.

Surry Road, 7th January 1804.

* If there be one at work near London, I have no doubt but Mr. Woolf the inventor, who is reſident engineer at Meſſrs. Meux's brewery, will readily ſhew it. N.

Farther

Farther Remarks on Mr. WOOLF'S *Rotatory Apparatus.*

Farther account of Woolf's rotatory apparatus.

IN order to render the effect of Mr. Woolf's contrivance (*Fig.* 1. *Plate* XI. Vol. VI. November 1803.) more intelligible to my readers in general, I ſhall here explain the ſame in a popular way.

I will ſuppoſe, by way of ſimplifying the ſubject, that the rod B is ſo long as to act always nearly in the perpendicular, and that the action upon the ſmall wheel is alſo in the ſame direction, namely, conſtantly that of gravity. The engraver has omitted to continue the rod of the piſton ſuppoſed to work in the barrel G, and the proof came too late for it to be altered in time. The effect at the pin F in the little wheel, is preciſely the ſame as if a weight was continually hanging there.

Effect of the power and weight in different poſitions.

1. In the poſition here ſhewn, the power of the engine does not act at all, and the weight at F has its full operation. —2. When C has moved through half the quarter circle to the right hand, F will have arrived at the loweſt point; conſequently the power of the engine will act by a lever equal to the ſine of 45°, or ſeven-tenths of the radius: or ſeven-tenths of the full power of the engine will then act on the great wheel, and the weight at F will not act at all: —3. When C has moved through an entire quadrant, the power of the engine will be the greateſt poſſible on the great wheel, but at the ſame time the weight will have arrived at the horizontal line beneath E, and will oppoſe the former action. The motive force will therefore be equal to the exceſs of the power of the engine beyond the weight. 4. When C has moved through another half quadrant, the action on the great wheel will be equal to ſeven-tenths, as in No. 2. and the weight, having arrived at the higheſt point will not act at all. 5. When C has moved through half a circle, and is at the lower ſtation in the great wheel the engine will not act, and the weight having arrived again at F. will have its full operation as in No. 1.

There are four poſitions in which the counter-weight has no effect;

By purſuing this ſimple method of examination, we ſee that 1. there are four poſitions in which the arm C ſtanding at an inclination of 45° to the horizon, and the arm F perpendicular to the ſame, the regulating weight can have no effect,

effect, whatever be its meaſure; the action will be effectual to ſeven-tenths of the direct or entire action of the arm B in the tangent to the great circle; 2. that when the arm C is either perpendicularly up or down, the regulating weight alone acts in the ſame tangent; 3. that when C lies horizontal, the effective force will be the exceſs of the power of the engine beyond that of the weight. Since therefore we can do nothing with regard to the four firſt mentioned poſitions, we have only to conſider the four laſt, and to contrive that the moving force ſhall be the ſame at the top and bottom, as at the two ſides; or in other words, that the weight alone ſhall be equal to the exceſs of the power of the engine beyond that weight; which is the ſame thing as ſaying, that the weight muſt be equal to half the power of the engine.

two in which the weight acts by itſelf; and two in which the effect is produced by the exceſs of the power beyond the weight.

The weight muſt be half the power.

We ſee then that the action on the wheel C, will vary from ſeven-tenths to five-tenths, four times in every revolution; the extreme difference being therefore two-tenths each way from the medium, which is about ſix-tenths. It may not perhaps be of any practical value to diſcuſs the gradation by which the increaſe and diminution of the oppoſite actions are governed; and the mathematical reader will eaſily ſee that the direct action of each is as the coſine of the angle of the obliquity of the radius it acts upon. Still leſs does it ſeem to be of any conſequence in the preſent looſe view of the ſubject, to treat of any aſſumed obliquity of the rods of communication.

Variation one tenth on each ſide the medium action.

If we ſuppoſe a power to act conſtantly and uniformly, but with alternations of oppoſite direction, in equal times and through like ſpaces; and wiſh to produce, by that action, a rotatory motion, we might in theory obtain the effect by a double ſpiral, known by workmen by the name of a ſnail: but the difficulties from friction, ſhake, and other impediments, would in many inſtances render the conſtruction ineligible. The crank is the moſt cheap, eaſy, light, and practicable means of producing rotation from alternate right-lined motion and the contrary; and its properties are too well known to require diſcuſſion in this place. Its inequalities demand a fly, whether the power or the reſiſtance be uniform or variable; and I do not ſee any ſtriking difference between Mr. Woolf's contrivance

Concerning the equalization of actions, &c.

Snail.

Crank.

trivance and the fly, as far as concerns one ſingle revolution, whether they be conſidered as regulating any one of theſe. In both, the ſurplus power is employed in putting a weight into a ſituation or ſtate by which it is enabled to give back (in theory) the ſame ſurplus, when the reſiſtance becomes greater or the power leſs. But if the whole power, in ſeveral ſucceſſive revolutions; ſhould exceed the reſiſtance, and produce acceleration, the fly will continue to accumulate momentum, and will be ready to give it out, againſt an augmented reſiſtance in ſucceeding revolutions; ſo that its equalizing power extends not only through each ſingle revolution, but through many. I do not perceive this property in the ingenious contrivance before us.

XII.

Obſervations on the extinct Volcanoes in the Environs of Coblentz, by the Chevalier DE SADE. *Tranſlated from the original Manuſcript, communicated by the Count de* BOURNON.

Coblentz built of volcanic ſtone, from the neighbourhood of Andernach.

WHEN I reſided at Coblentz in 1791, the volcanic ſtones made uſe of in that city, led me to inquire whence they were procured. I was ſoon informed, that they came from the neighbourhood of Andernach; and the clergyman of that place acquainted me, that the extinct volcanoes were near Haach, an abbey of Benedictine monks, three leagues diſtant. This worthy gentleman added, that he had ſomewhere ſeen it mentioned, that the Roman legions, encamped near Andernach, were obliged to quit their poſt, on account of the fire iſſuing from the volcano. Its great diſtance from the ſea, made me doubt the truth of this; and my own obſervations have convinced me, that the extinct volcanoes of this country are at length as ancient as thoſe of Auvergne, the date of which is unknown.

The Roman legions ſaid to have been driven from their camp near Andernach, by a volcano. This queſtionable.

The extinct volcanoes, are like thoſe of Auvergne, of unknown date.

Having procured a guide, and directing my ſteps toward the ſouth, I had ſcarcely quitted Andernach, before I met with river ſediment, intermingled with pumice ſtones and volcanic ſands. Leaving the high road, I proceeded toward a ſmall mountain on the weſt, called Agemberg.

Beyond Andernach, river ſediment intermixed with pumice ſtones and volcanic ſands. Agemberg, a ſmall mountain on the weſt.

To

To my great ſatisfaction, after having traverſed pumice ſtones and volcanic ſcoriæ, I arrived at the foot of a quarry of lava, from which hewn ſtones for building and millſtones were taken. This quarry was wrought in the open air, and was covered with a bed of mould about eighteen feet deep. The thickneſs of this ſtratum demonſtrates the high antiquity of the volcano, while at the ſame time it prevented me from tracing the courſe of the lava, particularly as above this lava, nothing is to be found but ſcattered blocks and fragments of lava, ſcoriæ, and pumice ſtones. I went to the top of the mountain, however, in hopes of diſcovering where the center might have been; for the mountain of Agemberg, all the rocks of which are torrefied, and the ſtate of ſcoriæ, was not a crater. The mountains, by which it is ſurrounded on the weſt, appeared to me to be of the ſame nature, without any indication of the mouth from which the fire iſſued.

A quarry of lava, from which hewn ſtones and millſtones were procured.

Wrought in the open air, and covered with eighteen feet of mould.

Above it ſcattered blocks and fragments of lava, ſcoriæ, and pumice ſtone.

Agemberg not a crater,

and the mountains, ſurrounding it, of the ſame nature.

The ſummit of mount Agemberg is very bare, but on deſcending a few paces, I found a very pleaſant copſe. A narrow path, which I took at a venture, led me, after ſome twenty ſteps, to a grotto, in which was a tomb, with an inſcription from Job, reminding man of his nothingneſs. By the ſide of this grotto is another, inhabited by a hermit, with a table of volcanic ſtone in the centre, which he uſes for different purpoſes. A little higher up, I diſcovered a third, ſmaller than either of theſe, containing a ruſtic altar; on one ſide was a pigeon-houſe, and lower down, two or three little baſons of water bordered with ſhells.

The top of Agemberg bare,

A little way down a pleaſant copſe, in which is a grotto, containing a tomb, another inhabited by a hermit, a third in which is an altar, a pigeon-houſe, and two or three baſons of water.

I quitted this mountain with regret, to traverſe another, which, though covered with wood, ſeemed to indicate the exiſtence of a crater; but I could perceive no trace of one, the trees, and the thickneſs of the mould, letting me ſee nothing but ſcattered blocks of lava and ſcoriæ.

Another mountain covered with trees and deep mould,

with ſcattered blocks of lava and ſcoriæ.

My examination was ſoon finiſhed, and I reſumed the road to the abbey of Haach. In this I found nothing but pumice ſtones, lava; in ſome places of the baſaltic kind, ſcoriæ, a few fragments of hornſtone ſchiſtus, and blocks of quartz; all ſcattered about on a ſtratum of earth, which itſelf was compoſed merely of river-ſand and pumice ſtone.

In the road to the abbey of Haach, pumice ſtones, lava, in ſome places baſaltic, ſcoriæ, hornſtone, ſchiſturs, and blocks of quartz,

on a ſtratum of river ſand and pumice ſtone.

The

The crater ſaid to be a lake near. Its ſituation fine.

The incumbent of the village belonging to the abbey, informed me, that the crater, of which I was in ſearch, was a lake near his pariſh.

On the weſt ſide firſt argil of different colours, then hornſtone, next ſcattered blocks of lava.

The ſituation of this lake diſplays much grandeur. It is ſurrounded by a chain of mountains covered with trees; its ſhape is oval, longeſt from north to ſouth; and it requires near two hours to walk round it.

The bank a poor meadow.

The weſt ſide offers to view at firſt nothing but a ſpacious bed of argil, divided into bands of differents colours; after this appears a ſmaller bed of hornſtone; and the reſt exhibits only ſome ſcattered blocks of lava. The bank is an indifferent meadow.

On the north, blocks of lava, then hornſtone cleft perpendicularly, and laſtly, a fat, white clay, interſperſed with blocks of lava and hornſtone.

The north ſide begins with ſome of the blocks of lava juſt mentioned: next appears a pretty extenſive bed of hornſtone, the very numerous fiſſures of which are uniformly perpendicular to the horizon: and a little farther on, is a conſiderable bed of fat clay, as white as ſnow, interſperſed with blocks of lava and hornſtone.

On the eaſt, compact and porous lava, and the mountain nearly perpendicular.

On approaching the eaſt ſhore, nothing is to be found but lava, in ſome places compact, in others porous, and the mountain there is nearly perpendicular.

On the ſouth a level meadow, and a ſhore covered with ſhells.

Laſtly, on the ſouth ſide, the ground becomes level, and is ſimply a meadow. The ſtrand is covered with ſhells, partly whole, partly broken; and hence I conclude, that the prevailing winds in this diſtrict, are the north and north-weſt.

The prevailing winds N. and N. W.

The lake has no apparent influx of water, but a rivulet runs from it on the ſouth.

This lake does not appear to me to receive water in any part: yet, on the ſouth it has an outlet, that furniſhes a ſmall brook, watering the grounds of Medermich. The depth of the lake varies greatly, being upwards of four hundred feet near the middle. It abounds with fiſh, particularly pike; but the blocks of ſtone ſcattered over its bottom, render the uſe of the net nearly impracticable, ſo that the line is almoſt excluſively employed for catching them.

Varies greatly in depth, more than 400 feet near the middle. Abounds with fiſh, but the bottom too rocky to uſe nets.

The abbey, to which this lake belonged, was a ſpacious building, proclaiming the opulence of its founder. It was built in 1093, by Henry count of the Palatinate of the Rhine, and lord of this lake. Broverus, in his annals of Treves, ſays, that the phantoms and lights, which this prince ſaw every night on the mountains around him, induced him, to erect this pious foundation.

The abbey built by the count Palatine in 1093, he being alarmed by the nocturnal appearance of fires on the mountains.

I was told, that the outlet I have juſt mentioned was artificial, and made fifty years after the building of the abbey. The foundations of this abbey, having been ſunk as deep as the outer walls are high above ground, had obſtructed the natural outlet, which occaſioned the water to riſe ſo high, as to inundate the convent, and render it neceſſary to form the preſent channel, in order that it might run off.

The preſent outlet artificial, the natural one having been obſtructed by the foundations of the abbey.

The face of the ground, however, makes me doubt, whether the lake ever flowed over on the ſide next the abbey; for to a conſiderable diſtance it riſes there uniformly as you proceed from the ſhore. It appeared to me, that the natural outlet muſt have been rather on the ſide toward Andernach, through fiſſures on the mountain. Neither can I believe, that this lake, if we may judge from its banks, was the crater of a volcano; but one of the great excavations produced by ſome earthquakes. In fact, I have already ſaid, that the eaſt and north-eaſt parts of this lake were volcanic: if therefore we ſuppoſe, that the ſummit of the mountain had been a volcano, its baſe muſt neceſſarily have become hollow, and in this ſtate of things, a very ſlight ſhock would have ſufficed to break the arch of the vault, or the cruſt that covered its abyſſes.

This queſtionable, as its natural outlet was more probably toward Andernach, through fiſſures in the mountain. Neither was it a crater, but probably produced by an earthquake.

We need not wonder, that this lake is always full of water, though it appears to receive none in any part; ſince it ſerves as a drain to the ſurrounding mountains, which are in great meaſure volcanic. Its water is very limpid, and it does not freeze except in very ſevere froſts. Marquart, in his origin of the Palatinate, aſſerts, that it was once larger than it is now; which in fact is perceptible on the ſouth and weſt, where the ground has the appearance of having been once covered with water. He adds, that precious ſtones and ſapphires were found on its borders. I did nor ſee any; but it is known, that there is a fact of this kind in the little rivulet of Pezzouliou, that runs among lava at Expilly, near Puy, in Velay.

Receives water from the ſurrounding mountains. Very clear, and does not eaſily freeze. Formerly larger than it is now. Sapphires and other precious ſtones, once found on its borders. A ſimilar occurrence at Expilly.

By no means ſatisfied with my reſearches, the principal object of which was, to diſcover the crater of the volcano, that could have produced all the lava in this country, I repaired to Medermenich, half a league to the ſouth-eaſt. There I found myſelf in quarries of volcanic ſtone, from which were taken mill-ſtones and hewn-ſtones for building, for

At Medermenick, quarries of volcanic ſtones.

for which there is a very great demand. Theſe quarries are underground, and in theſe are ſhafts a hundred feet deep, to facilitate the working.

Depth of the ſtrata.

The firſt ſtratum was mould, about 15 feet thick.

The ſecond, ſcorix of lava, about 12

Total 27.

Deſcent into them.

Beneath was the pure and compact lava. The deſcent into theſe quarries is by a ſtair of 120 ſteps, and a gentle deſcent of near 90 paces. The fragments of the ſtones got out ſerved to make internal walls of ſupport, to which were added ſtanchions of timber, in ſome places perpendicular, in others oblique, but always contrived to ſupport the blocks ſeparated by fiſſures. The ſtones were broken off by iron wedges, for they were afraid to ſeparate them by blaſting.

The roof ſupported by walls and timber.

The ſtones ſeparated by iron wedges.

The ſhafts numerous, and very dangerous from being left open.

I counted eighteen or twenty ſhafts in the ſpace I traverſed; and their mouths being perfectly open, woe to the traveller who ſhould loſe his way among them by night. I obſerved ſeveral of them covered up, indicating works abandoned; whence we may preſume, that theſe quarries had been wrought for ſeveral centuries. Some old oaks likewiſe growing in the ſhafts thus covered confirm this opinion.

Theſe quarries wrought for ſevera. centuries.

The thickneſs of the ſtrata covering this lava prevented me from following its courſe, and tracing it to its ſource; but I judged, that the mountain of Blemberg, a league to the ſouthweſt, might be its crater. Accordingly I proceeded to examine it. A quarter of a league from Medermenich I found a bed of the ſame lava, the face of which was perpendicular, and forming a gully, that might be twenty-five feet deep. It is covered by a very thin ſtratum of mould, and is wrought into millſtones, and ſtones for building. Being expoſed to the open air, it is harder than that of Medermenich.

Its crater probably at mount Blemberg.

Beyond Medermenich a bed of the ſame lava, with a perpendicular face, forming a ravine 25 feet deep, and covered with a thin ſtratum of mould.

This too wrought, and the ſtone harder from its expoſure to the air.

After having paſſed this gully, and a little bed of muddy lava, of the nature of piperino, we find in a meadow a very copious mineral ſpring. It is cold, and contains carbonic acid and iron. At ſome diſtance from this we meet with beds of lava, in which there had anciently been ſeveral quarries, and at length we arrive at the mountains of Blemberg. The ſurface of this mountain, though covered with trees on the eaſt, is nevertheleſs full of torrefied and ſcorified lava, and gray and red pouzzolona, which left me no doubt, that this mountain had formerly been a volcano. The inſpection of its ſummit

Beyond this, and ſome lava of the piperino ſpecies, is a cold acidulous chalybeate ſpring.

More lava in which were formerly quarries.

confirmed me in this opinion. Though divided into ſeveral parts, which do not perfectly correſpond with each other, that on the weſt perfectly repreſented a ſegment of a vaſt crater in good preſervation. Standing on the border of this ſegment, I obſerved on the eaſt, at the diſtance of half a league, quarries of lava, of which this mountain was the ſource. The gully I have mentioned ſeemed to militate againſt the adoption of ſuch an opinion with reſpect to the quarries of Medermenich, which are to the north-eaſt beyond theſe: but time, and the various circumſtances, that have formed fifteen feet depth of mould, may have occaſioned the apparent interruption of this current of lava. I do not doubt however, but ſeveral of the neighbouring mountains have been openings of volcanoes.

Blemberg covered with trees on the eaſt; but abounds with lava and pouzzalana.

Its ſummit once a vaſt crater, but partly deſtroyed.

Quarries of lava that proceeded from it.

As probably did thoſe of Medermenich.

But ſeveral of the neighbouring mountains were once volcanoes.

From the ſummit of Blemberg I enjoyed a grand and very extenſive proſpect. On the ſouth I had the little town of Mayerne, on the north Medermenich, and Ettringen on the weſt.

Proſpect from Blemberg fine.

I quitted this mountain to viſit that of Zimmeray, about half a league weſt of it, and I paſſed through the village of Ettringen, built of lava and ſcoriæ.

Village of Ettringen built of lava and ſcoriæ.

The whole of the body of the mountain of Zimmeray is volcanic. The lavas there are in a ſtate of torrefaction, that announces the exiſtence of a crater. In fact, when I reached its ſummit, I there found all the marks of one. It is of a conical form, and has a hollow opening to the ſouth. The ſides of this hollow facing the ſouth are well preſerved: thoſe that face the north have been deſtroyed. I imagine this took place at the period of the laſt eruption, and that a ſtream of lava would be found by digging on that ſide.

The mountain of Zimmeray volcanic.

Its ſummit a crater.

I then left this mountain, and proceeded to that of Calberg, which is on the north, and ſeparated from it only by a tolerably broad valley, in which I found trapp, of a blackiſh gray colour, in maſs, and in ſcattered blocks. At the foot of mount Calberg was a bed of black, coarſe grained, volcanic ſand, ſtratiform, and containing a great deal of mica in large leaves. The trees that covered its ſummit prevented me from aſcertaining its nature, but the circumference of its baſe is entirely volcanic. Around it is muddy lava, in which larger or ſmaller fragments of compact lava occur. Gray clay predominates there.

Mount Calberg contains trapp.

At its foot a bed of volcanic ſand, containing mica in large leaves.

Its baſe entirely volcanic.

Gray clay predominates.

Once a volcano.

I had no doubt, that this mountain was once a volcano, its figure alone proves it. I went beyond it to the quarries that were working, and which were to the north, at the foot of a gentle declivity, ſeparated from the Calberg by a valley, through which a ſmall brook flowed to the eaſt. Theſe quarries are parts of vaſt beds of that kind of lava, which the Italians call piperino. It is in general an earthy lava, containing a pretty conſiderable quantity of fragments of compact lava. This ſtone is ſoft, and eaſily wrought. It is almoſt every where in a horizontal ſtratum, but varies in its texture, for in ſome places it appears in the form of a volcanic breccia, in others it has the texture of ſand-ſtone.

Near it quarries of lava of the piperino ſpecies;

Soft and eaſily wrought; in ſome places a volcanic breccia, in others of a ſandſtone texture.

Having accompliſhed my object, which was to diſcover the volcanic openings, that furniſhed the lava of this diſtrict, I returned to the Medermenich, where I ſlept; and the next day I reached Coblentz. The plain I croſſed was covered with fragments of quartz, trapp, and pumice ſtone. Near Octundung, on the high road, we find rocks of ſlate, which are working. After leaving this village four gently ſloping mountains appear on the left, ſtretching from eaſt to weſt, and ſeparated only by narrow valleys. Their figure induced me to viſit them; and I found them compoſed of pumice ſtones and ſcoriæ. The firſt and ſecond are merely ſegments of two ſmall craters, the northern part of which has been carried away. The other two, which are covered with trees, appear to me to have formed but one crater of vaſt extent, the ſouthern part of which is deſtroyed. The lava of theſe little mountains is encruſted with an immenſe number of little cryſtals of piroxen. This reddiſh lava contains a pretty conſiderable quantity of reddiſh mica in large leaves, frequently half a line thick. Almoſt at the foot of the eaſternmoſt mountain this ſtone is procured, to be uſed in conſtructing walls, though it is too porous, and at the ſame time too hard, to be employed as hewn ſtone.

Road from Medermenich to Coblentz.

The plain covered with fragments of quartz, trapp, and pumice ſtone.

Near Octundung ſlate quarries.

Near it four volcanic mountains.

Their lava encruſted with ſmall cryſtals of piroxen.

It is reddiſh, and contains reddiſh mica in large leaves.

Uſed in walls.

From the ſummit of theſe heights the proſpect is beautiful and grand. From the eaſt to the north appears the magnificent baſin watered by the Rhine from the caſtle of Ehrenbreitſtein to Andernach. The ſeven mountains (Siebenberge) raiſe their ancient heads on the north. On the weſt, the view includes from north to ſouth all the mountains juſt deſcribed;

View from theſe heights.

and

and on the ſouth it is terminated by mountains covered with wood, the neareſt of which appeared to me to be volcanic.

At length I arrived at Baſnheim, remarkable for nothing but the Engliſh gardens belonging to the lord of the manor; and thence to Coblentz. I found pumice ſtone on the road, and in ſtrata where pits had been dug in the ground.

Engliſh gardens at Baſnheim. Pumice ſtone on the road and in ſtrata.

XIII.

On an Improvement in the Form of Spectacle-Glaſſes, by William Hyde Wollaston, *M D. F. R. S. Communicated by the Author.*

It muſt have been remarked by perſons who make uſe of ſpectacles, eſpecially thoſe who require glaſſes of ſhort focal diſtance, that objects ſeen through them appear diſtinct, only when viewed through the central parts of the glaſſes; that when the direction of the ſight E O, *Fig.* 1. page 145 is conſiderably inclined to the ſurfaces, objects appear diſtorted, and that this defect is greater in proportion to the greater obliquity of that line.

Imperfection of ſpectacles, that objects out of center are diſtorted.

It is on this account that opticians have lately made, and recommended ſpectacle-glaſſes of leſs diameter than thoſe formerly in uſe, thinking that the extreme parts of the field of viſion, which from indiſtinctneſs were of little uſe, might be ſpared without much inconvenience. But this alteration in the ſize of the glaſſes could hardly claim the merit of an improvement; ſince for one defect, it only ſubſtituted another ſcarcely leſs objectionable.

Remedy formerly propoſed; to make the glaſſes ſmaller;— but to little effect.

It ſeems indeed rather extraordinary, that, during five centuries which have elapſed ſince the invention of ſpectacles, neither theory nor accident ſhould have produced any conſiderable variation from the original conſtruction.

It was indeed conceived by Huygens, that the glaſſes, inſtead of being equally curved on both ſides, as is cuſtomary, ſhould have the curvatures of their oppoſite ſurfaces in the proportion of ſix to one, becauſe he had demonſtrated that ſuch a form was beſt ſuited to the object glaſſes of teleſcopes.

Huyghens propoſed that the radii of curvature in ſpectacle glaſſes ſhould be unequal.

Dr. Smith alſo in his treatiſe on optics, (p. 258.) repeats this opinion of Huygens in the following curſory manner;

Repeated by Smith.

and

" and conſequently this figure of a glaſs is the beſt for ſpectacles, as the double concave of like figure is the beſt to help ſhort-ſighted perſons."

This is good for ſimple object lenſes, but not for ſpectacles.

But although it may be very true that ſuch a form of glaſs was beſt calculated for the object glaſs of a teleſcope, previous to the celebrated diſcovery of the achromatic object-glaſs by the late by Mr. Dollond, yet whatever advantages might at any time be expected from the teleſcopic object-glaſs ſo ſhaped, theſe were not to be obtained by a ſimilar conſtruction in ſpectacles, as may eaſily be ſeen by conſidering the different uſes of the reſpective inſtruments.

Teleſcopic viſion is nearly in the axis :—

In a teleſcope in the firſt place, our view is neceſſarily confined to a very ſmall diſtance on each ſide of the axis; and ſecondly, every part of the object glaſs contributes to the diſtinctneſs of any object viewed.

and the whole lens acts for every radiant point.

It is under theſe circumſtances alone that the proportion of the curvatures above-mentioned, might be proper for a ſingle object glaſs, as being capable of collecting into the focus the rays that fall on every part of it parallel to the axis.

In ſpectacles it is very different.

By ſpectacles on the contrary, objects are to be viewed if poſſible in every direction in which they might be ſeen by the naked eye, which is often far removed from the centers of the glaſſes; conſequently a conſtitution that is calculated to repreſent correctly central objects alone, cannot be the moſt advantageous.

A ſmall part of the lens only is employed at once.

In theſe alſo, the portion of the glaſs employed at once is ſcarcely larger than the pupil of the eye, ſo that any endeavour to procure the concurrence of all parts of a glaſs in any one effect is evidently ſuperfluous, and may alſo be ſhewn to be prejudicial.

Whence improvement muſt be ſought from different conſiderations.

It is therefore propoſed to remedy the imperfections obſervable in the ſpectacle glaſſes hitherto generally uſed, upon a principle ſuggeſted by this latter conſideration, which preſents an opportunity by a different conſtruction of rendering objects in all directions diſtinct.

If the lens be made concave next the eye all the rays may be had to paſs, through nearly perpendicular.

The alteration requiſite for this purpoſe, is extremely ſimple, and eaſily intelligible. Suppoſing an eye to be placed in the center of any hollow globe of glaſs, it is plain that objects would then be ſeen *perpendicularly* through its ſurface in every direction. Conſequently the more nearly any

ſpectacle-

ſpectacle-glaſs can be made to ſurround the eye in the manner of a globular ſurface, the more nearly will every part of it be *at right angles* to the line of ſight; the more uniform will be the power of its different parts, and the more compleatly will the indiſtinctneſs of lateral objects be avoided *.

According to this principle, all ſpectacle-glaſſes ſhould be convex on their exterior ſurface, and concave within. The ſection of thoſe for long ſighted perſons, will aſſume the form of a meniſcus or creſcent, *Fig.* 2. and thoſe adapted for ſhort ſight will have their principal curvature on the concave ſide, *Fig.* 3.

Convex outſide and concave within.

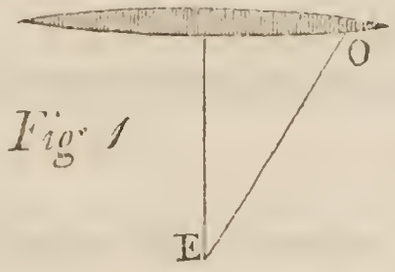


Fig. 1

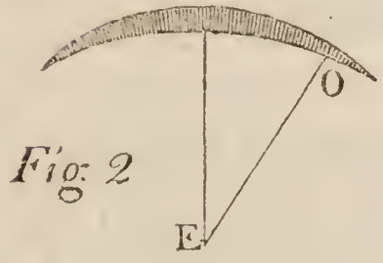


Fig. 2

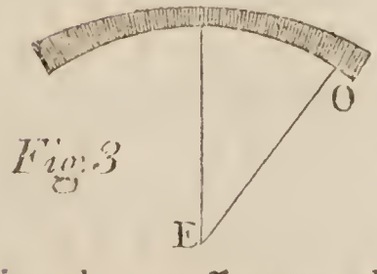


Fig. 3

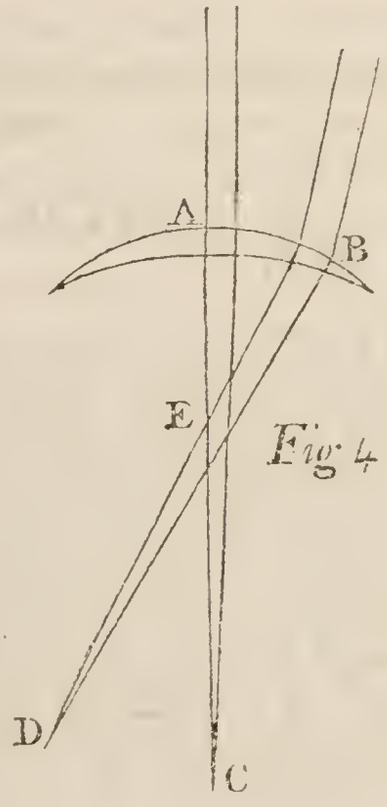


Fig. 4

It is only neceſſary to add, that the advantage of this improvement in the form of ſpectacle glaſſes has been confirmed by a ſufficient number of experiments on different perſons, and that thoſe in particular who are very long or very ſhort ſighted, are much benefited by them.

Experience confirms this doctrine.

* To mathematicians it will be evident that any ray which does not paſs through the center of a lens, cannot be at right angles to both ſurfaces; but they will alſo perceive that when any ſmall oblique pencil makes equal angles with the two ſurfaces of a thin lens, the inclination of it to each is ſo ſmall, that its focal length B D, *Fig.* 4. will not ſenſibly differ from A C that of a central pencil.

and a manufactory is eſtabliſhed.

The moſt advantageous proportions of curvature for obtaining the different focal lengths now generally diſtinguiſhed by certain numbers, have alſo been duly conſidered; and the manufacture of ſpectacles, on this conſtruction, has been undertaken by Meſſrs. P. and J. Dollond, to whom the excluſive ſale of them is ſecured by patent, and whoſe well known ſkill in the conſtruction of optical inſtruments, enſures to this improvement every advantage of correct execution.

The ſpectacles are called periſcopic.

From the opportunity afforded by theſe glaſſes of *looking round* at various objects in one poſition of the head, it is thought they may not improperly be diſtinguiſhed by the name of *Periſcopic Spectacles*.

XIV.

Letter of Enquiry concerning a Gun to throw double-headed Shot; with the Editor's Reply.

To Mr. NICHOLSON.

SIR,

Drawing and deſcription of a gun on a paper at the Royal Inſtitution.

HAVING this inſtant ſeen, at the Royal Inſtitution, a ſheet of paper printed by Nichols, printer, Earls Court, Newport Street, Soho, containing a drawing and deſcription of a gun to throw donble-headed ſhot, I take the liberty of writing this on a page of a little note-book there, which muſt be my apology for the paper, to entreat that you will obſerve upon it in your invaluable Journal, becauſe I fear that it is not a ſafe inſtrument. A conſiderable diſtance is to be obſerved between the powder fired and the ſhot to be diſcharged by it. I have always conſidered that an interval ſo kept was the occaſion of the burſting of ſo many guns. Pray is this ſafe? And ſhould not a line be cut on the arms to which the bullets are attached, to mark exactly the diſtance of the attaching bar at each extremity from the muzzle, when the balls are equally rammed down. As the knowledge of its ſafety may contribute to its ſervice if really uſeful, I hope you will think this application not idle.

I am, Sir,

Your conſtant READER.

ANSWER

ANSWER. W. N.

THE paper alluded to in the letter of my correſpondent, contains an engraving of the ſhaded ſection of a gun having a double bore, but only one chamber; from which each bore directly leads: but they diverge from each other in an angle of about eighteen degrees. The charge of powder is to be placed in the chamber, and the two ſhot, one for each bore, are connected by a bar having two joints or hinges at an equal diſtance from its middle. By this contrivance, by bending the compound piece at the hinges, the ſhot can be put into the bores of the gun; and when the exploſion blows them out, their divergence cauſes the bar to become ſtrait, and conſequently is expected to do miſchief proportioned to the extent of its length.

More particular account.

The title of the paper is, "Horizontal Section of a Gun for projecting double-headed Shot, invented by Lieut. Colonel Blaquiere, 22 Dragoons, July, 1803. Recommended to be uſed againſt an invading Army on board his Majeſty's Gun-Boats;" and in the directions care is preſcribed, that the ſame length of bar ſhall be left out of each muzzle to prevent accident; and it is ſtated to be a great advantage, that a conſiderable diſtance ſhould be left between the powder and ſhot, as by this means the powder acquires its greateſt poſſible force.

Title of the paper and inſtructions.

My opinion was aſked concerning a gun of this deſcription which another inventor had propoſed, ſome time previous to Midſummer laſt, to throw chain-ſhot; and the obſervations that then occurred were, 1. If there be the leaſt difference between the windages or the blaſt in each bore, or in the caſual impediments, one ſhot will precede the other. 2. If they go out fairly together, their velocity of ſeparation will be to the direct velocity as the chord of the angle of divergence is to the radius. In the preſent gun the balls will ſeparate with one-third of the velocity with which they advance. 3. That the violent ſeparation of the ſhot from each other, will moſt probably, in either of theſe caſes, break the chain, or the bar, or the joints. 4. Or if no fracture takes place, the ſudden check will, by the elaſticity of the metal, cauſe the ſhot to approach again to each other. 5. To which I may add, that it may be doubted whether the embarraſſment of one of theſe bar-ſhots preceding the other might not occaſion a dangerous obſtruc-

This gun invented alſo by another perſon.

Its probable effects.

tion, and that there are many facts which ſhew, that it is unſafe to permit any conſiderable expanſion of, or ſpace for, the blaſt of powder, before it ſhall act on the projectile.

XV.

Prognoſtics of the Weather, eſtabliſhed by long-continued Obſervation upon the Conduct and Appearances of Birds, Beaſts, Inſects, Plants, Meteors, the Heavenly Bodies, Minerals, &c. Communicated by a Correſpondent.

To Mr. NICHOLSON,

SIR,

Introduction. Prognoſtics of the weather.

IT is a well known general fact, that ſhepherds and others, whoſe occupations lie in the open air, do not unfrequently poſſeſs the ſkill of foretelling the weather for conſiderable periods of time in advance, and that they ground their obſervations upon the phenomena exhibited by animals, and other bodies expoſed to the action of the elements. Some of the maxims on this ſubject, which poſſeſs the ſanction of ancient acquieſcence, are ſuch as probably would not ſtand the teſt of modern ſcientific examination; but on the other hand, there are many of which the rationale appears not difficult to explain, and others which, from their empyrical value, are highly deſerving to be ſtudied and made out. I have for theſe reaſons, as well as from a ſenſe of the immediate utility of this knowledge, thought it by no means impertinent to beg that you would oblige the world with the encloſed, which is the beſt collection of facts I have met with. It is taken from a ſmall pamphlet printed at Edinburgh, without date or the name of bookſeller or author, but apparently many years ago, intitled, A Succinct Treatiſe of Popular Aſtronomy. If this communication ſhould meet the honour of inſertion in your excellent Miſcellany, I hope it will be followed by ſome explanations from your ſcientific correſpondents.

I am, Sir,

Your obliged reader,

R. B.

Signs

Signs of Rain from Birds.

Prognoſtics of the weather. Rain from birds.

SEA and freſh water-fowls, ſuch as cormorants, ſea-gulls, muir-hens, &c. flying from ſea, or the freſh waters, to land, ſhew bad weather at hand: land fowls flying to waters, and thoſe ſhaking, waſhing, and noiſy, eſpecially in the evening, denote the ſame: geeſe, ducks, coots, &c. picking, ſhaking, waſhing, and noiſy; rooks and crows in flocks, and ſuddenly diſappearing; pyes and jays in flocks, and very noiſy; the raven or hooded-crow crying in the morning, with an interruption in their notes, or crows being very clamorous at even; the heron, bittern and ſwallow flying low; birds forſaking their meat and flying to their neſts; poultry going to rooſt, or pidgeons to their dove-houſe; tame fowls grubbing in the duſt, and clapping their wings; ſmall birds ſeeming to duck, and waſh in the ſand; the late and early crowing of the cock, and clapping his wings; the early ſinging of wood-larks; the early chirping of ſparrows; the early note of the chaffinch near houſes; the dull appearance of robin red-breaſt near houſes; peacocks and owls unuſually clamorous.

Signs of Wind from Birds.

Wind from birds.

Sea and freſh water-fowls gathering in flocks to the banks, and there ſporting, eſpecially in the morning; wild geeſe flying high, and in flocks, and directing their courſe eaſtward; coots reſtleſs and clamorous; the hoopoe loud in his note; the king's-fiſher taking to land; rooks darting or ſhooting in the air, or ſporting on the banks of freſh waters; and laſtly, the appearance of the malefigie at ſea is a certain forerunner of violent winds, and (early in the morning) denotes horrible tempeſts at hand.

Signs of Fair Weather from Birds.

Fair weather from birds.

Halcyons, ſea-ducks, &c. leaving the land and flocking to the ſea; kites, herons, bitterns and ſwallows flying high and loud in their notes; lapwings reſtleſs and clamorous; ſparrows after ſun-riſe reſtleſs and noiſy; ravens, hawks and keſtrils (in the morning) loud in their notes; robin red-breaſt mounted high, and loud in his ſong; larks ſoaring high, and loud in their ſongs; owls hooting with an eaſy and clear note; bats appearing early in the evening.

Signs

Signs of Rain from Beaſts.

Rain from beaſts.

Aſſes braying more frequently than uſual; hogs playing, ſcattering their food, or carrying ſtraw in their mouths; oxen ſnuffing the air, looking to the ſouth, while lying on their right ſides, or licking their hooves; cattle graſping for air at noon; calves running violently and gamboling; deer, ſheep, or goats, leaping, fighting or puſhing; cats waſhing their face and ears; dogs eagerly ſcraping up earth; foxes barking, or wolves howling; moles throwing up earth more than uſual; rats and mice more reſtleſs than uſual; a grumbling noiſe in the belly of hounds.

Signs of Rain from Inſects.

Rain from inſects.

Worms crawling out of the earth in great abundance; ſpiders falling from their webs; flies dull and reſtleſs; ants haſtening to their neſts; bees haſtening home, and keeping cloſe in their hives; frogs and toads drawing nigh to houſes; frogs croaking from ditches; toads crying on eminences; gnats ſinging more than uſual; but, if gnats play in the open air, or if hornets, waſps, and glow-worms appear plentifully in the evening, or if ſpiders webs are ſeen in the air, or on the graſs, or trees, theſe do all denote fair and warm weather at hand.

Signs of Rain from the Sun.

Rain, from ſun.

Sun riſing dim or wateriſh; riſing red with blackiſh beams mixed along with his rays; riſing in a muſty or muddy colour; riſing red and turning blackiſh; ſetting under a thick cloud; ſetting with a red ſky in the eaſt.

N. B. Sudden rains never laſt long; but when the air grows thick by degrees, and the ſun, moon, and ſtars ſhine dimmer and dimmer, then it is like to rain ſix hours uſually.

Signs of Wind from the Sun.

Wind from the ſun.

Sun riſing pale and ſetting red, with an iris; riſing large in ſurface; riſing with a red ſky in the north; ſetting of a bloody colour; ſetting pale, with one or more dark circles, or accompanied with red ſtreaks; ſeeming concave or hollow; ſeeming divided, great ſtorms; parhelia, or mock ſuns, never appear, but are followed by tempeſts.

Signs

Signs of Fair Weather from the Sun.

Fair weather from the ſun.

Sun riſing clear, having ſet clear the night before; riſing while the clouds about him are driving to the weſt; riſing with an iris around him, and that iris wearing away equally on all ſides, then expect fair and ſettled weather; riſing clear and not hot; ſetting in red clouds, according to the old obſervation:

The evening red and morning grey,
Is the ſure ſign of a fair day.

Signs of Rain from the Moon.

Rain, from the moon.

Moon pale in colour, rain; horns blunt at firſt riſing, rain; horns blunt, at or within two or three days after change, denotes rain for that quarter; an iris with a ſouth wind, rain next day; wind ſouth third night after change, rain next day; the wind ſouth, and the moon not ſeen before the fourth night, rain moſt of that month; full moon in April, new and full moon in Auguſt, for moſt part, bring rain; mock-moons are the forerunners of great rains, land-floods, and inundation.

Signs of Wind from the Moon.

Wind from the moon.

Moon ſeeming greatly enlarged; appearing of a red colour; horns ſharp and blackiſh; if included with a clear and ruddy iris; if the iris be double, or ſeem to be broken in parts, tempeſts.

N. B. On the new moon, the wind for the moſt part changes.

When the moon, at four days old, has her horns ſharp, ſhe foretels a tempeſt at ſea, unleſs ſhe has a circle about her, and that too entire, becauſe, by that ſhe ſhews that it is not like to be bad weather, till it is full moon.

Signs of Fair Weather from the Moon.

Fair weather from the moon.

Moon ſeeming to exhibit bright ſpots; a clear iris with full moon; horns ſharp fourth day, fair till full; horns blunt at firſt riſing, or within two or three days after change, denotes rain for that quarter; but fair weather the other three quarters; moon clear three days after change or before full, always denotes fair weather. After every change and full, rains for the moſt part, ſucceeded by fair ſettled weather; moon clear and bright, always fair weather.

Signs

Signs of Weather from the Stars.

Weather from ſtars.

Stars ſeeming large, dull, and pale of colour, rain; or when their twinkling is not perceptible, or if encompaſſed with an iris. In ſummer, when wind is at eaſt, and ſtars ſeem greater than uſual, then expect ſudden rain; ſtars appearing great in number, yet clear and bright, ſeeming to ſhoot or dart, denote fair weather in ſummer, and in winter froſt.

Signs of Rain from the Clouds.

Rain from clouds.

In cloudy weather, when the wind falls, rain follows; clouds growing bigger, or ſeeming like rocks or towers ſetling on mountains tops; coming from the ſouth, or often changing their courſe; many in number at north weſt in the even; being black in colour from the eaſt, rain at night; but out of the weſt, rain next day; being many like fleeces of wool, from the eaſt, rain for two or three days; lying like ridges about mid-day in the ſouth-weſt, ſhews great ſtorms both of wind and rain to be nigh.

Signs of Wind from the Clouds.

Wind from clouds.

Clouds flying to and fro; appearing ſuddenly from the ſouth or weſt; appearing red, or accompanied with redneſs in the air, eſpecially in the morning; being of a leadiſh colour in the north-weſt; ſingle clouds denote wind from whence they come; but if at ſun-ſet, clouds appear with golden edges, or diminiſh in bulk, or ſmall clouds ſink low, or draw againſt the wind, or appear ſmall, white, and ſcattered in the north-weſt (ſuch as are vulgarly called mackerel) when the ſun is high, theſe are ſigns of fair weather.

N. B It is often obſerved, that though the mackerel ſky denotes fair weather for that day, yet for the moſt part, rain follows in a day or two after.

Signs of Rain from the Rainbow.

Rain from rainbow.

After a long drought, the rainbow denotes ſudden and heavy rains; if green be the predominant colour, it denotes rain, but if red, wind with rain; if the clouds grow darker, rain; if the bow ſeems broken, violent ſtorms; if appearing at noon, much rain; if in the weſt, great rain, with thunder.

N. B.

N. B. It is obſerved, that if the laſt week in February, and the firſt fortnight of March, be moſtly rainy, and attended with frequent appearances of the bow, a wet ſpring and ſummer may be expected.

Signs of Fair Weather from the Rainbow.

The rainbow appearing after long rains, denotes fair weather at hand; if the colours grow lighter, fair; if the bow ſuddenly diſappears, fair; if the bow appear in the morning, it is the ſign of ſmall rains, followed by fair weather; if appearing at night, fair weather; if appearing in the eaſt, in the evening, fair; if the bow appear double, it denotes fair weather at preſent, but rain in a few days; if in autumn, it continues fair for two days after the appearance of the aurora borealis, expect fair weather for at leaſt eight days more. Fair from rainbow.

Signs of Rain from Miſts.

If miſts be attracted to the tops of hills then expect rain in a day or two; if in dry weather, they be obſerved to aſcend more than uſual then expect ſudden rain; miſts in the new moon always foreſhew rain in the old; miſts alſo in the old moon denote rains to happen in the new; a miſty white ſcare, in a clear ſky, in the ſouth-eaſt is always a forerunner of rain. Rain from miſts.

Signs of Fair Weather from Miſts.

If miſts diſſipate quickly, or deſcend after rain, it is a ſure ſign of fair weather; a general miſt before ſun riſing near the full moon, denotes fair weather for about a fortnight running. If after ſun ſet or before ſun riſe, a white miſt ariſe from the waters and meads, it denotes warm and fair weather next day. A miſty dew on the inſide of glaſs windows ſhews fair weather for that day. Fair from miſts.

Signs of Rain from inanimate Bodies.

Wood ſwelling, or ſtones ſeeming to ſweat; lute or viol ſtrings breaking; printed canvas or paſted maps relaxing; ſalt becoming moiſt; rivers ſinking, or floods ſuddenly abating; remarkable ſparkling of lamps or candles; remarkable halo about the candle; great dryneſs of the earth; pools ſeeming troubled or muddy; yellow ſcum on the ſurface of ſtagnant waters; dandelion or pimpernel ſhutting up; treſoil ſwelling in ſtalk, while the leaves bow down. Rain from inanimate bodies.

N. B. A dry ſpring is always attended with a rainy winter.

Signs

Signs of Wind from inanimate Bodies.

Wind from inanimate bodies.

Winds ſhifting to the oppoſite point; ſea calm, with a murmuring noiſe; a murmuring noiſe from the woods and rocks; when the air is calm; leaves and feathers ſeeming much agitated; tides high when the thermometer is high; trembling or flexuous burning of flames; coal burning white with a murmuring noiſe; thunder in the morning with a clear ſky; thunder from the north.

N. B. Whenſoever the wind begins to ſhift, it will not reſt till it comes to the oppoſite point; and, if the wind be in the north, it will be cold; if in the north-eaſt colder; if in the ſouth; it brings rain; but if in the ſouth-weſt more rain.

Signs of Rain ceaſing.

Rain ceaſing.

The ſudden cloſing of gaps in the earth; the remarkable riſing of ſprings or rivers; if the rain begins an hour or two before ſun riſe it is like to be fair ere noon; but if an hour or two after ſun-riſe, it for the moſt part, happens to continue all day and then to ceaſe; when it begins to rain from the ſouth with a high wind for two or three hours, and that the wind falls, and it ſtill continues raining, it is then like to continue for twelve hours or more, and then to ceaſe.

N. B. Theſe long rains ſeldom happen to hold above twenty-four hours, or happen above once a year.

Signs of Wind ceaſing.

Wind ceaſing.

A haſty ſhower after raging winds is a ſure ſign of the ſtorm being near an end. If the water ruckles and frequent bubbles ariſe, or if the halcyon or kings-fiſher attempts the ſea while the ſtorm laſts, or moles come out of their holes, or ſparrows chirp merrily, theſe are all certain ſigns of the ſtorm ceaſing Both ſea and freſh water fiſhes by their frequent riſing and fluttering on the ſurface of the water, foretell the ſtorm nigh over, but eſpecially dolphins ſpouting up water in a ſtorm foretell a calm.

N. B. Let the wind be in what quarter it will upon the new moon, it preſently changes.

Signs of Hail.

Hail.

Clouds white, inclining to yellow, and moving heavily though the wind be high is a ſure ſign of hail; if the eaſtern ſky before ſun-riſe be pale, and refracted rays appear in thick clouds,

clouds, then expect great ſtorms of hail; white clouds in ſummer are a ſign of hail, but in winter they denote ſnow, eſpecially when we perceive the air to be a little warm; in ſpring or winter when clouds appear of a blueiſh white, and expand much, expect ſmall hail or drizzling, which properly is no other than frozen miſts.

Signs of Thunder.

Meteors ſhooting in the ſummer's evening, or chops and clefts in the earth, when the weather is ſultry, always foretell thunder is nigh; in ſummer or harveſt, when the wind has been ſouth two or three days, and the thermometer high, and clouds riſe with great white tops like towers, as if one were upon the top of another and joined with black on the nether ſide, expect rain and thunder ſuddenly; if two ſuch clouds ariſe, one on either hand, it is then time to look for ſhelter, as the thunder is very nigh. Thunder.

N. B. It is obſerved that it thunders moſt with a ſouth wind and leaſt with an eaſt.

Signs of Cold and Froſty Weather.

Sea-pyes, ſtarlings, fieldfares, with other migratory birds appearing early, denote a cold ſeaſon to enſue; the early appearance of ſmall birds in flocks and of robin red-breaſts near houſes; ſun in harveſt after ſetting in a miſt or broader than uſual; moon bright, with ſharp horns, after change; wind ſhifting to the eaſt or north after change; ſky full of twinking ſtars; ſmall clouds hovering low in the north; ſnow falling ſmall, while clouds appear on heaps like rocks. Cold and froſty.

N. B. Froſts in autumn are always ſucceeded with rain.

Signs of Thaw.

Snow falling in large flakes while the wind is at ſouth; cracks appearing in the ice; ſun looking wateriſh; the moon's horns blunted; ſtars looking dull; wind turning to the ſouth; wind extremely ſhifting; it is alſo obſerved, that, if October and November be froſt and ſnow, January and February are like to be open and mild. Thaw.

Signs of Drought.

Fair weather for a week together, while the wind is all that time in the ſouth, is, for the moſt part, followed by a great drought; Drought.

drought; if February be for moſt rainy, ſpring and ſummer quarters are like to be ſo too; but if it happen to be altogether fair, then expect a drought to follow; if lightning follow after twenty-four hours of dry and fair weather, drought will follow, but if within the twenty-four hours, expect great rains.

Signs of Hard Winters.

Hard winters. A moiſt and cold ſummer, and mild autumn, are ſure ſigns of a hard and ſevere winter; ſtore of hips and haws denote the ſame; the hazel-tree flowering is ever obſerved to foretell the ſame; acorns found without any inſect is a ſure prognoſtic of a hard winter.

Signs of Peſtilential Seaſons.

Peſtilential ſeaſons. A dry and cold winter with a ſoutherly wind; a very rainy ſpring, ſickneſs in ſummer; if ſummer be dry with the wind northerly but the autumn rainy and the wind ſoutherly, great ſickneſs is likely to follow; great heats in ſpring time without winds; roots having a luſcious taſte, while the wind has been long ſoutherly without rain; and laſtly, great quantities of ſtinking atoms, inſects or animals, as flies, frogs, ſnakes, locuſts, &c.

Experiments of the Leech Worm.

Leech worm. Incloſe the leech worm in an eight ounce vial glaſs, three fourths filled with water covered with a bit of linen, let the water be changed once a week in ſummer, and once a fortnight in winter.

If the leech lies motionleſs at the bottom in a ſpiral form, fair weather; if crept to the top, rain; if reſtleſs, wind; if very reſtleſs, and without the water, thunder; if in winter at bottom, froſt; but, if in the winter it pitches its dwelling on the mouth of the vial, ſnow.

Signs of Weather from the Barometer.

Barometer. In calm weather, when the air is inclined to rain, the mercury is low; but when tending to fair, it will riſe; in very hot weather when falling, it foreſhews thunder; if riſing in winter, froſt; but, if falling in froſt, thaw; if riſing in a continued froſt, ſnow; if foul weather quickly on its falling, ſoon over; if fair weather quickly on its riſing, ſoon over; alſo if riſing high in foul weather, and ſo continuing for two or three days,

before

before the foul weather is over, then expect a continuance of fair weather; but, if in fair weather the mercury fall low, and ſo continue for two or three days, then expect much rain, and probably high winds.

N. B. In an eaſt wind, the mercury always riſes and falls loweſt before great winds.

XVI.

A New, Cheap and Simple Apparatus for repeating the Hours, and Quarters in Clocks and Watches. By Mr. J. M. Elliot. *Communicated by the Inventor.*

New and ſimple repeating motion.

THE great expence of repeating watches in the firſt purchaſe, as well as in the ſubſequent repairs, have induced Mr. Elliot to turn his thoughts towards reducing their conſtruction to a greater degree of ſimplicity, and conſequently to diminiſh the charges in both theſe reſpects.

In the mechaniſm repreſented in plates VII. and VIII. the parts and their arrangement are ſuch that without the uſual apparatus of pinions, pullies, chains, or racks, the watches repeat both the hour and the quarters, viz. the hour firſt and the quarter afterwards, or the hour or the quarters ſeparately; or either of theſe firſt as may be wiſhed, without interruption, and by one hammer. As ſeveral of theſe pieces have been made and diſpoſed of in this country as well as exported, the practicability as well as the coſt are both aſcertained, which laſt is about one third of the former charges.

Plates VII. and VIII. repreſent the parts of this new machinery, *Fig.* 1. ſhews the principal pieces as attached to the upper pillar plate by means of a pottance A B C D, called the repeating pottance, kept to its place by the ſcrew *x*.—*a b* ſhews the pendant attached to a ſteel axis upon which the repeating apparatus is fixed and carried. In the actual time piece theſe parts are as cloſe to each other as convenience will allow; but they are here repreſented for the ſake of diſtinctneſs, as if at a diſtance from each other. Thus they are made to act and perform the ſtriking by turning the axis of the pendant to the right or left.

c is a part of the bell ſeen edgewiſe, *d* and *l* are two arms or pallets, which being fitted upon ſquares on the axis are

New and ſimple repeating motion.

moved round whenever the axis is moved by means of the pendant. The firſt *d* ſerves to drive the work for ſtriking the hour, and the latter *l* the work for the quarters. The firſt of theſe apparatuſes conſiſts of *c* the hour-locking ſnail (ſee alſo *Fig.* 2.) which is fitted on the axis by a round ſocket or pipe, upon which ſocket alſo is fixed (ſteadily) the hour repeating wheel *g* (*Fig.* 1 and 2,) and (on a round part) the ratchet wheel *h*, which laſt is drawn by a ſpiral ſpring *k k* in the direction oppoſite to that which is produced by the pallet *d* (*Fig.* 1 and 2). *s* is the hammer urged by its ſpring *v* outwards, and the deſigner has, I am ſorry to perceive, omitted to make a ſmall tail from the center pin near *k* towards *g* the hour repeating wheel. Now the action of ſtriking is ſimply this; when by turning the pendant *d* acts upon the ſmall pin in *e*, the wheel *g* with its ratchet wheel *h* are carried round, and the teeth of *g* act on the tail of the hammer, and give more or fewer ſtrokes, according to the number that paſs.

The number is regulated thus, *a* (*Fig.* 4.) is the ſtar-wheel as uſual, and *b* the hour ſnail ſcrewed on the ſame; *c* is the click with its back ſpring, and *n z* is the hour-locking lever, which terminates in an hook at *z* that may be ſeen in *Fig.* 2. bearing againſt the hour-locking ſnail *e*. This hook will evidently be nearer to or farther from the axis of the locking-ſnail *e*, according to the portion of the other end of the lever which bears againſt the hour ſnail *b*, and will conſequently ſtop the pendant in its motion ſooner or later by its oppoſition to one of the ſtops of the ſnail *e*. As the ſtar wheel *a* is carried regularly round by the canon pinion as uſual, its ſituation will determine the number of ſtrokes.

The inſtant the pendant is let go, the ſpring *k* acts upon the ratchet wheel, and reſtores the original ſituation ready for ſtriking again.

After this account of the hours, the ſtriking of the quarters will require little explanation, *l* (*Fig.* 1 and 5.) is the pallet or arm acting (by a reverſe motion of the pendant,) on the locking ſnail *m*, which carries the contrate wheel *p*, for ſtriking the quarters by its action, upon another tail *r* of the hammer; and the original ſituation is reſtored by a ratchet wheel O and its ſpring, (ſee *Fig.* 6.)

In *Fig.* 4. is ſeen the quarter ſnail *g* with its lever *h k*, whoſe hook for regulating the number, may be ſeen at D in *Fig.* 5. and the ſnail is prevented from going back by a fixed hook.

SCIENTIFIC NEWS.

Prize for the Artificial Compoſition of Palladium.

IN conſequence of the intimation received by the anonymous paper of which a copy is given in the laſt number of this Journal, at page 75, I waited upon Mrs. Forſter, who perſonally aſſured me that ſhe holds the ſum of twenty pounds, (which was paid to her by the ſame unknown perſon who placed the palladium in her hands,) under the truſt or engagement to pay the ſame to the bearer of the certificate mentioned on the ſaid paper, at any time before Midſummer next; and that, if not claimed during that time, ſhe ſhall afterwards conſider herſelf at liberty to repay the money when demanded by the ſame unknown perſon.

I have therefore requeſted Charles Hatchett, Esq. F. R. S. and Edward Howard, Esq. F. R. S. to join myſelf as judges of the product which may be made in our preſence, agreeably to the before-mentioned paper; to which propoſal they have conſented. And I have no doubt but that the commiſſion will enable me to preſent to my readers an account of whatever may be the reſult of this public invitation.

Prize adjudged at the laſt Public Meeting of the National Inſtitute.*

THE decree of the government, May the 2nd. 1803, that anthorized the Inſtitute to accept the capital of 10000 franks, [417 £.] offered by Lalande, expreſſes, "that, agreeably to the intention of the donor, the intereſt ſhould be laid out annually in a gold medal, which, or its value in money, ſhould be given to the perſon, in France or elſewhere, the members of the Inſtitute only excepted, who ſhould have made the obſervation, or written the memoir, that might appear moſt conducive to the progreſs of aſtronomy."

Annual prize for the moſt uſeful aſtronomical paper or obſervation.

Accordingly, on the report of the committee nominated for the purpoſe, the phyſical and mathematical claſs decreed the prize to Dr. W. Olbers, for having diſcovered, in the courſe of the year 10, the planet to which aſtronomers have given the name of *pallas olberſiana*.

Decreed to Dr. Olbers for the diſcovery of a planet.

* *La Decade Philoſophique*. No. 29. July 1803. p. 142.

the

The Dr's. merit as an aſtronomer.

Before this diſcovery Dr. Olbers enjoyed conſiderable reputation among aſtronomers, for a diſſertation on the moſt eaſy and commodious method of calculating the orbit of a comet; and for the laborious calculations and obſervations he had made, to re-diſcover Ceres, or Piazzi's planet, which in fact he did perceive, and was the firſt to announce to us.

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*Teylerian Society at Haarlem.**

Teylerian ſociety's prize queſtion.

THIS ſociety has propoſed the following queſtion:

"Has the application of pretended ſupernatural principles to phyſics contributed to the progreſs of the ſcience? or does not the hiſtory of this ſcience prove on the contrary, that all the progreſs made in natural philoſophy has been owing to obſervations, experiments, the concluſions drawn from them, and mathematical calculations and demonſtrations?"

The prize is a gold medal of the value of 400 florins (£40), and the anſwers to the queſtion, written in Dutch, Latin, French, Engliſh, or German, muſt be ſent to the ſociety before the 1ſt of April, 1804.

*** *The explanation of the ſubjects of the queries of R. B. in our laſt, (page 71.) are deferred for want of room.*

* *La Decade Philoſophique*, No. 33. Auguſt, 1802. p. 382.

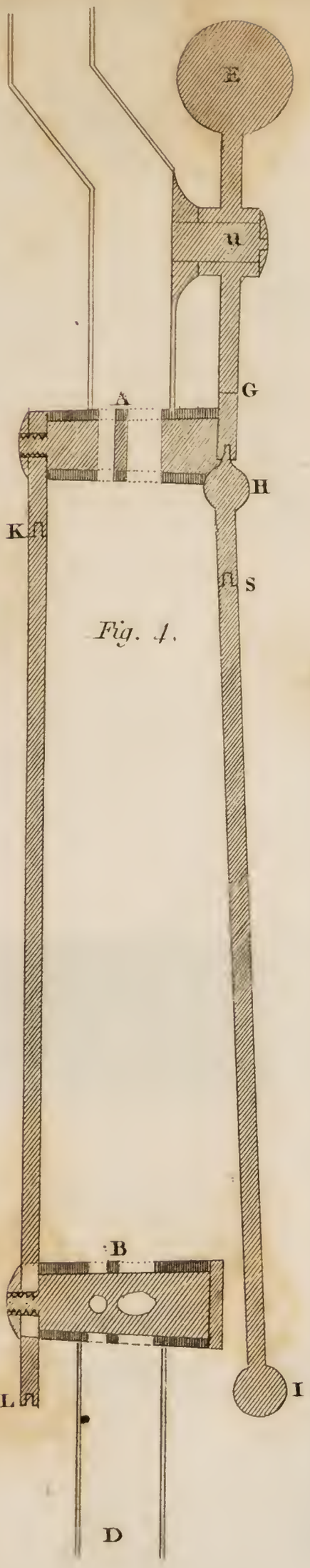


Fig. 4.

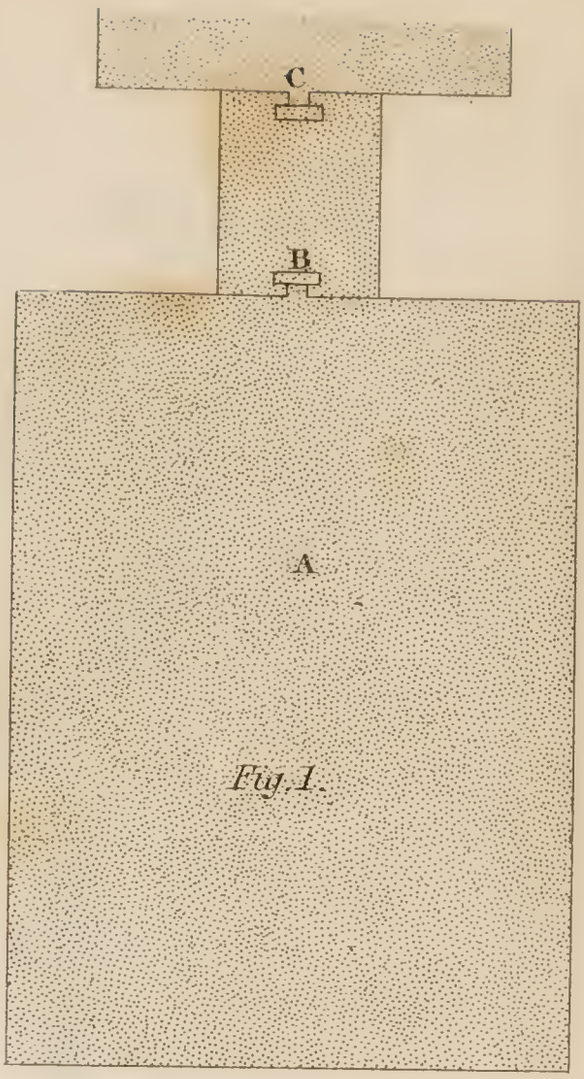
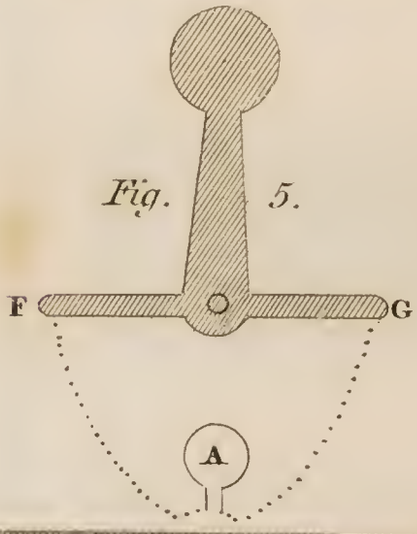


Fig. 1.

Fig. 5.



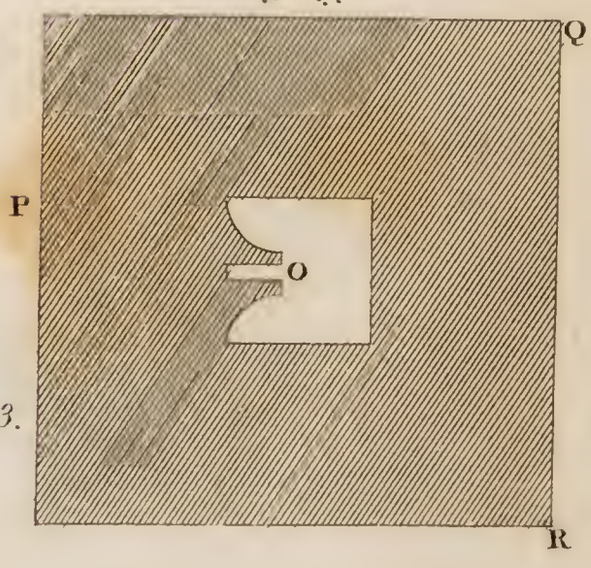


Fig. 3.

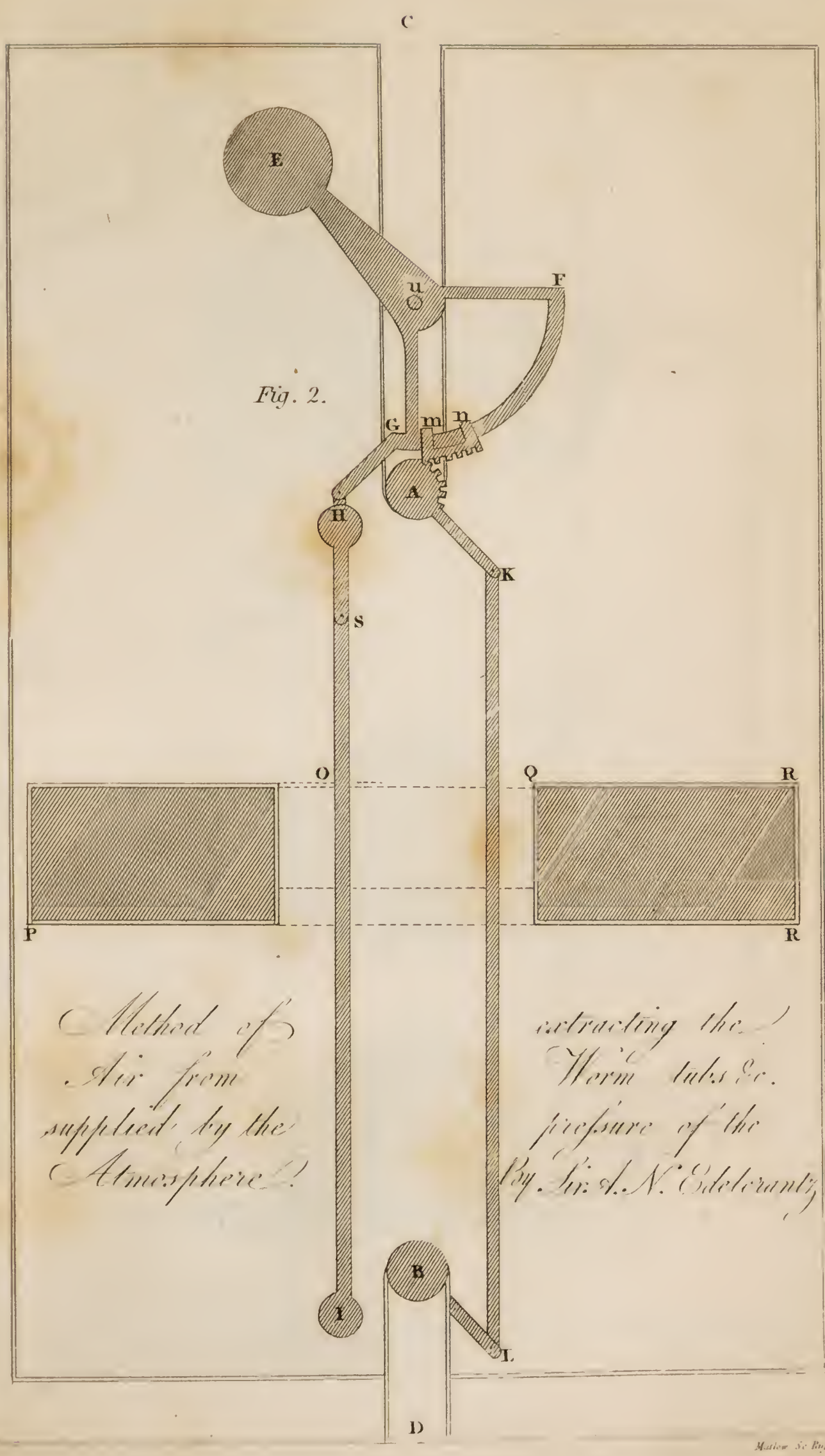


Fig. 2.

Method of extracting the Air from Worm tubes &c. supplied by the pressure of the Atmosphere.

By Sir A. N. Edelcrantz

Matlow Sc Rufsell

New & Simple Repeater, by Mr. L. H. Elliot.

Fig. 1.

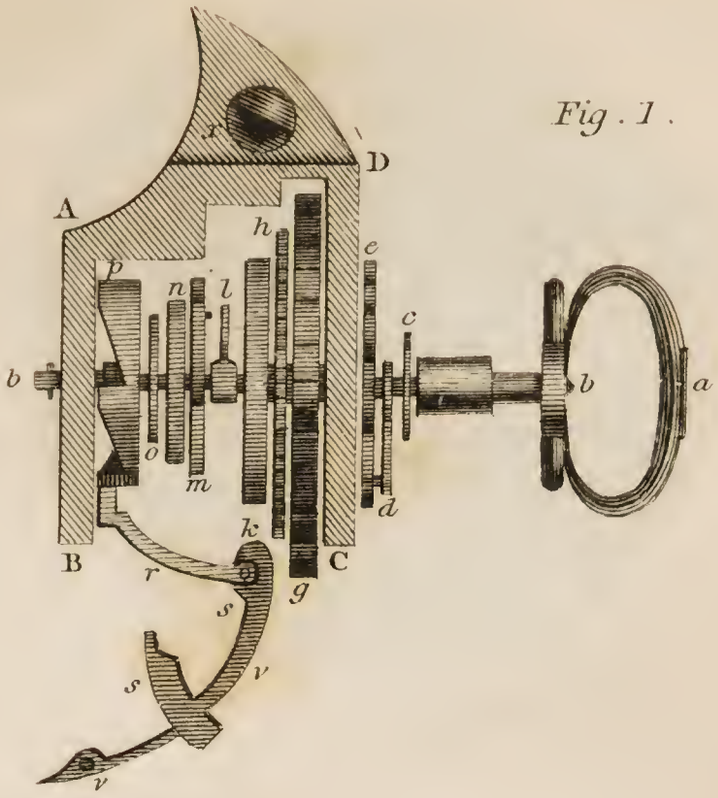


Fig. 2.

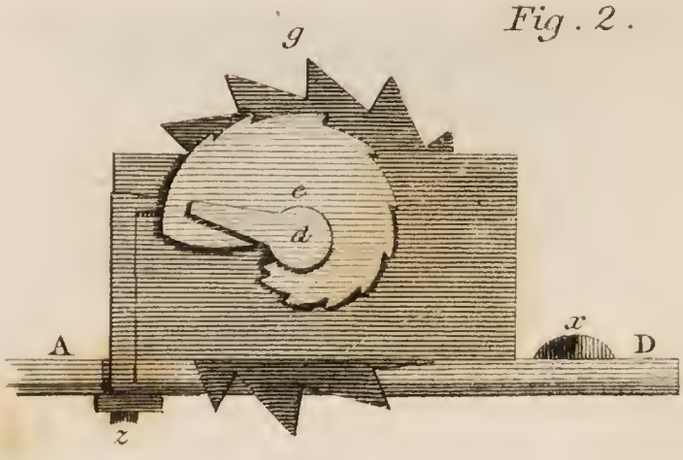
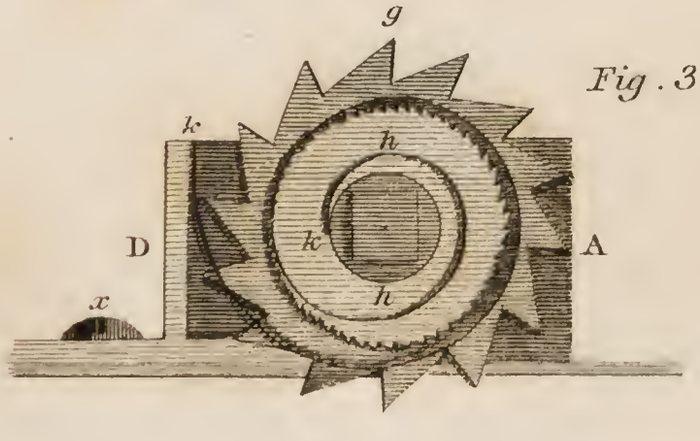


Fig. 3.



A

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OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1804.

ARTICLE I.

Deſcription of a new Steam Digeſter for Philoſophical Reſearches. By Sir A. N. EDELCRANTZ, *Counſellor of the Chancery, and private Secretary to the King of Sweden; Member of the Swediſh Academy, &c. &c. Communicated by the Author.*

THE intereſting and curious phenomena which this machine preſented, from its firſt diſcovery by Papin, have always rendered it a deſirable part of the chemical apparatus; but the difficulties and dangers attending its uſe, have, at the ſame time, prevented its applications from being ſo general, as the importance of the object ſeemed to promiſe. Many, and ſome of them ſucceſsful, attempts have been made to adapt this inſtrument to economical and culinary uſes; but theſe being commonly limited within very few degrees of heat above the boiling point, ſuch digeſters, though they can be uſed with ſafety, are at leaſt inaccurate in the permanence, and uncertain in the determination of the elaſticity and heat of the vapours acting in each experiment, which conſequently cannot be either preciſe or comparable with others: and though ſeveral ingenious philoſophers, ſuch as Watt, Betancourt, Smith of Geiſſen, Van Marum, &c. have in later times very much improved their ſcientific application, enough of imperfection ſtill ſeems to remain to prevent their general uſe, and to afford ſufficient ſcope for further improvements.

Excellence and utility of Papin's digeſter.

Improvement.

As long ago as the year 1793 I applied my thoughts to render this inſtrument leſs imperfect, and though I cannot flatter myſelf with having ſucceeded in any conſiderable degree, a ſhort account of my attempts may not perhaps be thought quite uſeleſs.

The uſual method of cloſing the digeſter is faulty in many reſpects.

My firſt object was to obtain a perfect cloſure for the digeſter, which is neceſſary not only for increaſing the heat, but alſo for preſerving the contents. In former contrivances this has generally been effected by a metallic cover preſſed on the top of the veſſel by means of ſcrews, wedges or other mechanical contrivances, with a circular piece of leather, paper, or other ſoft material between both. I alſo tried theſe methods in a great variety of ways, but always found ſuch covers either leaky, on applying moderate preſſures, or when the preſſure was ſtronger, too cumberſome for a courſe of varied and repeated experiments. Beſides which, at the degree of 260 or 270° Fahr. the leather, as obſerved before by Mr. *Betancourt*, is commonly decompoſed or burnt, and linen or paper ceaſe to cloſe at a ſtill lower temperature; as ſoon as the moiſture, they are imbibed with becomes converted into vapour. I therefore uſed another cover of a particular kind, conſiſting of a thick circular plate, with its edge turned conical, and ground to fit exactly in the under ſide of a metallic ring of the ſame conical form, folded to the mouth of the digeſter, in which the plate was previouſly included. When the veſſel was ready for experiment and the plate lifted up ſo as to apply to the ring, it cloſed as exactly as could be deſired, and the increaſed force of the ſteam, which tends to render other covers defective, had in this a contrary effect. Two difficulties were thus overcome, that of a cumberſome preſſing apparatus, and the inſufficiency of packings; but as the circular plate in this conſtruction was made to remain conſtantly in the digeſter, I found that though it was laid near the ſide of it, in ſome experiments this mode was attended with an inconvenience which I was deſirous of removing. I ſucceeded by making another circular ring, turned with both ſides conical, to be put between the circular plate and the opening of the digeſter, fitting exactly to each of them. This ring *a b*, *Fig. 5. Plate* IX. paſſes over the cover *c d*, when both are lowered, the ring remaining in the digeſtor; but the circular plate is taken out through the enlarged opening *e f*. Though I had

New cover which fits by a conical interior ſurface, without packing.

Inconvenience of this contrivance.

I had by this contrivance,* in a great meaſure diminiſhed the inconveniences, yet the intermediate ring remaining in the veſſel, and the increaſed difficulty of executing a more complicated inſtrument, urged me to attempt ſome other more expeditious mode of cloſing.

Simple and moſt accurate cloſure.

I ſhould almoſt be aſhamed to confeſs that the moſt ſimple and, as I preſume, the beſt contrivance of all, did not occur to me before I had tried the more complicated ones; if many reſpectable examples had not proved that the eaſieſt and ſhorteſt method is often the moſt difficult to find. The reaſon is obvious: Simplicity, like truth, is only one; the bye paths of error are innumerable.

Deſcription of the ſame. It is a plate which fits againſt a ſurface within.

This method conſiſts in making the cover ſimply an oblong, ſquare, or oval plane braſs plate, accurately ground againſt the inferior ſurface of a ſimilar frame of the ſame metal, fixed and ſoldered to the opening of the digeſter. This frame has an oblong aperture, through which the cover may be put in or taken out at pleaſure. When put in and preſſed gently againſt the frame, it will by the means of a little oil,† have adheſion enough to bear its own weight, till the ſteam begins to act, which will cloſe it perfectly. But for greater ſecurity a ſmall piece of wood, fixed with a ſcrew, will keep the plate joined to the frame, and ſerve at the ſame time as a handle. *Fig.* 1. *Plate* IX. repreſents the longitudinal ſection of the whole inſtrument, with all its parts; *a b* is the covering plate, ground and fitted againſt a ſquare metallic frame *c d*, ſoldered ‡ to the veſſel at *e f*, which ought to be made of hammered copper at leaſt $1\frac{1}{2}$ line thick. *Fig.* 2 is the tranſverſe ſection of the ſame apparatus, and *Fig.* 3. a plan of the braſs plate, where the dotted lines mark the opening of the frame *c d*, *Fig.* 1. and the

* The digeſter with a conical plate, and the ſafety-piſton deſcribed below, firſt executed in Stockholm, 1793, was exhibited to the members of the Royal Society in Berlin, in Feb. 1802; and a deſcription of it, printed in the Obſervations Phyſiques of Mr. Delametherie, Feb. 1803.

† The different force of adheſion of the ſame metal, in employing different oils, is remarkable, and may afford a field for future inveſtigation.

‡ Beſides ſoldering, the parts ſhould be joined together with rivets or ſcrews.

part covered by the ſame. *Fig.* 4. *h i* is the wooden handle turning round the ſcrew *g*, *Fig.* 1, 2. to connect the metallic planes. It is a kind of button or key.

Peculiar ſafety valve by which the preſſure may be regulated.

My next object was to render the inſtrument ſafe, and to place the operator's mind independent of any apprehenſion of fatal accidents. For this purpoſe, (beſides the ſtrength of the veſſel and its different parts, which ought to be as ſolid as conveniently can be made,) a metallic cylinder *m n* is fixed to the plate *a b*, to the inferior open extremity of which a metallic ſphere *m b*, perforated with ſmall holes, adheres by mere friction, allowing the vapour, but not the ſolid or other matters, to paſs into the cylinder, which contains a ſolid piſton *k o*, made of the ſame metal * and exactly ground into it, ſo as to ſink down by its own weight if lifted, without however permitting the vapour to eſcape. This vapour when heated to a certain degree, will raiſe the piſton above one or more of the capillary holes *s s*, on the ſide of the cylinder and then eſcape. The piſton will remain ſtationary or oſcillating at a height, where the quantity of ſteam eſcaping is equal to the quantity produced in the ſame time by that fire, conſequently at a low point if the fire is weak, or, if the fire is intenſe, at the top, where a larger hole preſents a compleat iſſue to the ſteam and prevents the poſſibility of accidents.

Conditions for accurate experiment with the digeſter. 1. To know the temperature. 2. To command the heat, and 3. To render it ſteady though the fire be not ſo.

To adapt the inſtrument thus conſtructed to exact and comparative philoſophical experiments, three things ſeem to be required: 1. to aſcertain the degree of heat of the interior fluid contents † by a thermometer; 2. to increaſe that heat at pleaſure; and 3. to fix it invariably, if it be deſired, independent of the poſſible increaſe of the fire, during the courſe of the experiment.

The temperature meaſured by a thermometer.

The firſt is obtained by means of a ſmall iron cup *p q*, ſoldered into the plate, and containing ſome mercury. A fine thermometer paſſed through a cork, being fixed in the cup, the

* Great care ſhould be taken to make both of the ſame metal, for in one inſtance when I had the piſton made of the ſame braſs with the cylinder, but of another day's caſting, this infinitely ſmall difference was ſtill ſufficient to produce a different force of expanſion, in conſequence whereof the piſton, which moved freely at 212°, ceaſed to move, and was quite faſtened at 260°.

† It is perhaps ſuperfluous to obſerve that a digeſtor is only intended for experiments with fluids or bodies ſurrounded with them, and conſequently never contains materials in a dry ſtate.

degree

degree of heat is eaſily obſerved on the ſtem. This degree will not be exactly the ſame with that of the interior ſpace; it ſhews always ſomewhat leſs, which fact was long time ago obſerved by Mr. *Braun* of the academy of Peterſburg; but the difference is ſmall, and being once aſcertained, by experiment made on purpoſe, a due correction, by adding that difference, is eaſily made.

The heat is communicated in my apparatus by a common Argand's lamp, which was always found ſufficient and of which the effect may be regulated by the pinion of that inſtrument. If time be an object, or if the digeſter is much more than double the ſize of *Fig.* 1. the ſcale of which is one third of the real ſize, two ſuch lamps may be applied. But a lamp with ſpirit of wine and three wicks will ſupply fire enough for a temperature of 270 or 280 if the digeſter be ſmall and exactly cloſed. As the atmoſphere continually takes away a part of the heat communicated to the veſſel, it is only the difference of the communicated and diſſipated heat which acts on the ſteam, and determines the limits of its force, when the fire is weak, and the digeſter of a large ſize. On the other ſide, the heat of the interior fluid and the elaſticity of ſteam correſponding to it, cannot, after having lifted the piſton, be encreaſed by a continued fire or by a greater number of lamps, but remains from that moment conſtant, which ſeems to be a particular property of this conſtruction. A farther *increaſe* of heat, as the 2d. deſideratum, can only be obtained by oppoſing a greater reſiſtance to the action of the vapour beneath the ſurface *k*, which is performed by loading the piſton with one or more of the weights *r r*, *Fig.* 1. Thus we may either add new weights progreſſively, till the thermometer indicates the degree of heat required, or knowing by the former experiments of philoſophers the correſponding ratio of increaſed preſſure and heat of elaſtic fluids, we can load the piſton at once with a weight proportional to the intended heat and the ſurface of its baſe *k*. This will then riſe as ſoon as that degree is produced, and allowing the ſteam to eſcape from one or more of the holes * prevent

The heat given by a lamp.

The heat is conſtant if the preſſing weight be not changed.

What is the ſecond condition.

* By what is ſaid above it is evident that the height of the piſton, or the number of the holes open to the ſteam, is of no conſequence, it being always compreſſed by the ſame weight, and acting with the ſame elaſticity. The *quantity* produced is only different.

any

any further increaſe of the heat, which will remain *invariable* as the third deſideratum required.*

Very great diſadvantage of imperfect cloſure of the digeſtor.

Here it ſeems proper once again to inſiſt upon the great importance of a perfect cloſure in this inſtrument. If the heated ſteam, by inaccuracy in the conſtruction, is permitted to eſcape, not only a great part of the contents is waſted without any uſe, before the experiment can be ſaid to begin, which ſometimes obliges us to open the digeſter and fill it a ſecond time, but even ſome of the materials may be altered and decompoſed before the intended effect can take place. Still more, very often in ſuch caſes, a ſtrong kitchen fire, with the greateſt exertions of the operator, (not to mention the trouble and inconvenience of ſuch an operation) is inſufficient to afford the degree of heat, which a ſingle lamp, placed with the apparatus on a ſmall writing table, would produce with eaſe, and without waſte of the materials.

In the preſent ſtate of natural philoſophy, the progreſs of the ſcience ſeems to depend in a great meaſure on the perfection of our inſtruments. In this point of view the preceding attempts to render a little uſed but very excellent machine more ſafe, exact, and convenient †, may, if it ſhould not anſwer that purpoſe, at leaſt give riſe to other more ſucceſsful improvements.

* A ſhort deſcription of this contrivance was preſented to the *Philoſophical Magazine* in June, 1803, and publiſhed in December the ſame year; but by the uſe of the word *piſton* inſtead of *digeſter* in a note, an erroneous ſtatement is given in the printed account, which, however trifling it may appear, it is proper to correct, as this part of the inſtrument has not been changed at all, ſince it was firſt made and known in Sweden ten years ago. In the ſame paper I propoſed the uſe of it for regulating the fire in boilers of ſteam engines, &c.—and its effect as a *regulator*, both of the *power* of the *ſteam*, and the *intenſity* of the *fire*, depends entirely upon the *weight* being conſtant and *invariable* in the whole motion of the piſton.

† Digeſters, as deſcribed in this paper, are made by Mr. FIDLER, mathematical inſtrument maker, 23, Oxford-market, London.

II.

Inveſtigation of certain Theorems relating to the Figure of the Earth. By JOHN PLAYFAIR, *F. R. S. Edin. and Profeſſor of Mathematics in the Univerſity of Edinburgh.*

(Concluded from Page 116.*)*

21. A REMARK, that is in no danger of being reckoned hypothetical, is, that the concluſion derived from the compariſon of degrees of the meridian, with degrees of the circle perpendicular to it, becomes of neceſſity more liable to error as we advance into higher latitudes. The reaſon is, that whatever error is committed in determining the magnitude of $D' - D$, muſt be multiplied into the ſquare of the ſecant of the latitude, in order to give its full effect in changing the value of the fraction $\frac{c}{a}$. For it has been ſhewn that $\frac{c}{a} = \frac{1}{2}\left(\frac{D' - D}{D'}\right) \text{ſec}^{\,3}\phi$; now, if we ſuppoſe the error committed in aſcertaining $D' - D$ to be in all caſes the ſame, the error of the fraction $\frac{D' - D}{D'}$ will alſo be in all caſes nearly the ſame, the denominator D' being but little affected either by the ſuppoſed error, or by the change of latitude. But this error, which may thus be conſidered as a conſtant quantity, when multiplied into $\frac{1}{2}$ ſec $^2\phi$, gives the variation or error in $\frac{c}{a}$, which error therefore increaſes, *cæteris paribus*, as the ſquare of the ſecant of the latitude, ſo that, on approaching the pole, it increaſes without limit, and is ultimately infinite. Compariſons of this kind may therefore be expected to give reſults the more accurate the nearer they are to the equator, under which circle they will be the moſt accurate of all. Here, again, however, another circumſtance muſt be taken into conſideration, viz. that the method of aſcertaining the differences of longitude by the convergency of the meridians, ſo convenient in ſurveys of this kind, is applicable only in high latitudes. In a trigonometrical ſurvey, therefore, of a country lying much farther ſouth than Britain, a different method of aſcertaining the longitudes of places muſt neceſſarily be adopted.

This method by the meridian and its perpendicular is more erroneous the higher the latitude.

22. The

To determine the figure of the earth from meaſures of the perpendicular in different latitudes.

22. The theorems, which were next propoſed to be conſidered, are thoſe that determine the figure of the earth from the meaſures of degrees of the curve perpendicular to the meridian, in different latitudes. For this purpoſe let D' be a degree of one of theſe curves, in the latitude φ', and D'' a degree of one of them, in another latitude φ''. Then c being the compreſſion, as before, we have by § 18. $mD' = a + c \sin^2\varphi'$,

and alſo $mD'' = a + c \sin^2\varphi''$,

Hence $m(D' - D'') = c(\sin^2\varphi' - \sin^2\varphi'')$, and

therefore $c = \frac{m(D' - D'')}{\sin^2\varphi' - \sin^2\varphi''}$.

This formula may be rendered more convenient for calculation, by conſidering that $\sin^2\varphi' = \frac{1 - \cos 2\varphi'}{2}$, ſo that

$\sin^2\varphi' - \sin^2\varphi'' = \frac{1 - \cos 2\varphi' - 1 + \cos 2\varphi''}{2} = \frac{\cos 2\varphi'' - \cos 2\varphi'}{2}$. But $\cos 2\varphi'' - \cos 2\varphi' = 2 \sin(\varphi' + \varphi'') \times \sin(\varphi' - \varphi''$, wherefore $\sin^2\varphi' - \sin^2\varphi'' = \sin(\varphi' + \varphi'') \times \sin(\varphi' - \varphi'')$, and $c = \frac{m(D' - D'')}{\sin(\varphi' + \varphi'') \times \sin(\varphi' - \varphi'')}$.

23. In the ſame manner, becauſe $mD' = a + c \sin^2\varphi'$, by ſubſtituting for c, we have

$$mD' = a + \frac{m(D' - D'')\sin^2\varphi}{\sin(\varphi' + \varphi'') \times \sin(\varphi' - \varphi'')}, \text{ and}$$

$$a = mD' - \frac{m(D' - D')\sin^2\varphi}{\sin(\varphi' + \varphi'') \times \sin(\varphi' - \varphi'')}.$$

24. Laſtly, ſince $mD' = a + c \sin^2\varphi'$,

and $mD'' = a + c \sin^2\varphi''$,

dividing the firſt of theſe equations by the ſecond, and rejecting the higher powers of c, we have

$\frac{D'}{D''} = 1 + \frac{c}{a}(\sin^2\varphi' - \sin^2\varphi'')$, and therefore,

$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\sin^2\varphi' - \sin^2\varphi''}$. Hence alſo

$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\sin(\varphi' + \varphi'') \times \sin(\varphi' - \varphi'')}$; or more conveniently for calculation by logarithms, $\frac{c}{a} = \frac{D' - D''}{D'' \sin(\varphi' + \varphi'') \times \sin(\varphi' - \varphi'')}$.

25. We

25. We may compare this value of $\frac{c}{a}$ with that obtained in § 18. from other data, in order to determine which of the two methods of finding $\frac{c}{a}$ is to be preferred, under given circumſtances. Suppoſe, for inſtance, a degree of the curve perpendicular to the meridian, in the latitude φ' to be D′, and a degree of the meridian itſelf in the ſame latitude to be Δ'; it is required to find in what other latitude φ'', a degree D″, perpendicular to the meridian, muſt be meaſured, in order that the compariſon of D′ and D″, and of D′ and Δ, may give values of $\frac{c}{a}$, in which the probable error is the ſame.

To determine the figure of the earth from meaſures of the perpendicular in different latitudes.

Here, agreeably to an obſervation already made, we may, in order to eſtimate the error produced in $\frac{c}{a}$, in conſequence of an error in the determination of D′, and D″, and Δ, ſuppoſe the error to affect D′ − D″, or D′ − D″ only, without paying any regard to the variation of D′ in the denominator. Therefore, ſince by § 18 we have $\frac{c}{a} = \frac{D' - \Delta}{2D' \text{ cof}^2\varphi'}$, and again by § 24, $\frac{c}{a} = \frac{D' - D''}{D'' (\text{ſin}^2\varphi' - \text{ſin}^2\varphi'')}$, if we ſuppoſe equal errors in determining D′ − Δ, and D′ − D″, and alſo that theſe are the only errors, their effect will be the ſame, in both caſes, if $2 \text{ cof } \varphi'^2 = \text{ſin } (^2\varphi' - \text{ſin } ^2\varphi'')$. Now, if we ſuppoſe φ'' the quantity ſought, and add $\text{cof } \varphi'^2$ to both ſides of the preceding equation, then $3 \text{ cof}^2\varphi' = \text{ſin}^2\varphi' + \text{cof}^2\varphi' - \text{ſin}^2\varphi'' = 1 - \text{ſin}^2\varphi'' = \text{cof}^2\varphi''$. The latitude φ'' therefore muſt be ſuch, that $\text{cof } \varphi'' = \sqrt{3} \times \text{cof } \varphi'$. If, therefore, φ' be ſuch that $\text{cof } \varphi' = \frac{1}{\sqrt{3}}$, the coſine of φ'' will be = 1, and φ'' therefore = 0. Now, 54° 44′ is the arch of which the coſine = $\frac{1}{\sqrt{3}}$ nearly, therefore if a degree of the meridian, and of the perpendicular to it, be meaſured in latitude 54° 44′, the compariſon of theſe with one another will give a reſult as accurate as if the degree of the perpendicular, in that latitude, were compared with the degree at the equator, and more accurate of conſequence, than if any other degree of the perpendicular to the meridian, were to be compared with D′.

26. Hence,

To determine the figure of the earth from meaſures of the perpendicular in different latitudes.

26. Hence, alſo, the compariſon of the degree of the meridian, and of the perpendicular to it, in the ſouth of England, is better than if a degree of the perpendicular meaſured in that latitude were compared with a degree at the equator. For if, in the equation $\text{cof } \varphi'' = (\text{cof } \varphi') \times \sqrt{3}$, we make $\varphi' = 50^\circ\ 41'$, (or any thing leſs than $54^\circ\ 44'$,) φ'' will come out impoſſible.

27. It may be ſhewn, too, nearly in the ſame manner, that if a degree of the perpendicular to the meridian were meaſured in Siberia, as far north as the latitude of 70°, ſuppoſing that to be poſſible, and compared with a degree in latitude 45°, or even conſiderably farther ſouth, it would not give a reſult ſo exact as the degree of the meridian and perpendicular meaſured in the ſouth of England. This ſhews, that the method of aſcertaining the figure of the earth, propoſed by the authors of the *Trigonometrical Survey, (Phil. Tranſ.* ibid. p. 529) as a ſubject of future inquiry, is leſs exact than that which is founded on their own obſervations.

Whether the compariſon of a degree of the merid. and perp. in ſame lat. be more accurate than that of merid. degs. in different lats.

28. We may alſo aſcertain, by the ſame means, the relative accuracy of the method of finding the figure of the earth, from the compariſon of a degree of the meridian with a degree of the perpendicular in the ſame latitude, and of the method of reſolving the ſame problem by the compariſon of two degrees of the meridian in different latitudes.

If, then, D be a degree of the meridian, and D′ of the perpendicular, in latitude φ, and if Δ be a degree of the meridian in a different latitude φ', it is required to find whether the moſt accurate value of $\frac{c}{a}$ will be found, by comparing D and D′, or D and Δ.

Since we have, by what has been already ſtated, § 4.

$$mD = a - 2c + 3c \text{ ſin } {}^2\varphi, \text{ and}$$

$$m\Delta = a - 2c + 3c \text{ ſin } {}^2\varphi', \text{ we have alſo}$$

$$\frac{D}{\Delta} = 1 + \frac{3c}{a} (\text{ſin } {}^2\varphi - \text{ſin } {}^2\varphi') \text{ and therefore,}$$

$$\frac{c}{a} = \frac{D - \Delta}{3\Delta\ (\text{ſin } {}^2\varphi - \text{ſin } {}^2\varphi')}.$$

Now, it has been already ſhewn, that, by comparing D and D′ we have $\frac{c}{a} = \frac{D' - D}{2D' \text{ cof } {}^2\varphi}$. Suppoſing, therefore, equal errors to be committed in the determination of $D - \Delta$, and of

D′

$D' - D$, and alſo paying no regard to the inequality of Δ and D' in the denominators of theſe fractions, as it is not ſo great as materially to affect the quantity that is ſought for here, we ſhall have the errors in $\frac{c}{a}$ nearly the ſame in both formulas, when φ and φ' are ſuch that $2 \operatorname{cof}^2\varphi = 3 \operatorname{ſin}^2\varphi - 3 \operatorname{ſin}^2\varphi'$, or when $\frac{2}{3} \operatorname{cof}\varphi^2 = \operatorname{ſin}^2\varphi - \operatorname{ſin}^2\varphi'$, that is, adding $\operatorname{cof}^2\varphi$ to both ſides,

$\frac{5}{3} \operatorname{cof}^2\varphi = \operatorname{ſin}^2\varphi + \operatorname{cof}^2\varphi - \operatorname{ſin}^2\varphi'$, and, therefore,

$\frac{5}{3} \operatorname{cof}^2\varphi = 1 - \operatorname{ſin}^2\varphi' = \operatorname{cof}^2\varphi'$, or $\operatorname{cof}\varphi' = (\operatorname{cof}\varphi)\sqrt{\frac{5}{3}}$.

29. If, therefore, $\operatorname{cof}\varphi = \sqrt{\frac{3}{5}}$, $\operatorname{cof}\varphi' = 1$, that is $\varphi' = 0$, ſo that Δ, the ſecond of the degrees of the meridian, muſt in this caſe be under the equator. But $\sqrt{\frac{3}{5}}$ is the coſine of $39^\circ\ 14'$, in which latitude therefore if D and D' be meaſured, the reſult, by comparing them with one another, is as exact as if D were compared with the degree under the equator. Hence, if D and D' are meaſured in a lower latitude than the above, the reſult will be more exact, than if D were compared with the degree at the equator.

If we ſuppoſe D and D', meaſured in the ſouth of England, ſo that $\varphi = 50^\circ\ 41'$; then we will have $\varphi' = 35^\circ\ 7'$, ſo that D muſt be compared with a degree of the meridian as far ſouth as $35^\circ\ 7'$, in order that the reſult may be as good as when D and D' are compared with one another.

From this it is evident, that the method of comparing degrees of the meridian, and perpendicular in the ſame latitude, has even an advantage over the compariſon of degrees of the meridian in different latitudes, unleſs theſe laſt are taken at a conſiderable diſtance from one another.

The former method is preferable unleſs the degs. of the mer. be far aſunder.

In this way may many uſeful concluſions be derived concerning the degree of credit due to meaſurements already made, as well as with reſpect to the ſelection of the places where they are to be made hereafter. On theſe I ſhall enter no further at preſent, and ſhall only add, that, beſides the advantages or diſadvantages which the method of comparing together degrees of the meridian and perpendicular in the ſame latitude has, and which are ſubjects of calculation, it has another

The former method alſo ſuppoſes the ſame obſervers and the ſame inſtruments.

other advantage, which in the caſe of the Britiſh ſurvey is undoubtedly very great, viz. that all the *data* are furniſhed from one ſyſtem of trigonometrical operations; executed according to the ſame plan, with the ſame inſtruments, and by the ſame obſervers.

To determine the earth's figure by comparing an arc of the mer. with that of a parellel in ſame lat.

30. One other application of geometrical meaſurements to diſcover the figure of the earth yet remains to be conſidered. This is the compariſon of an arch of the meridian with an arch of a parallel of latitude which croſſes it. The meaſure of a parallel of latitude can be executed readily, and is not confined to a ſmall arch, as in the caſe of a perpendicular to the meridian. The plumb-line, while it is carried along the circumference of a parallel to the equator, tends continually to the ſame point in the earth's axis, ſo that there is no difficulty in aſcertaining the amplitude of the arch meaſured, providing there be no unuſual diſturbance of the direction of gravity. As an arch of a parallel to the equator, however, is not the ſhorteſt line between two points on the ſurface of the ſpheroid, the meaſurement along that ſurface will not give the length of the arch truly. To obviate this difficulty, it is only neceſſary to follow the method ſo properly introduced into the *Trigonometrical Survey*, of reducing the meaſures, both of lines and angles, to the chords and to the planes of the rectilineal triangles contained by them. In this way, the chord of an arch of a parallel of latitude may be determined, however great the arch; and it is worthy of being remarked, that, whatever be the deflections of the plumb-line at the intermediate ſtations, when the reductions are all properly made, the length of the chord meaſured will not be affected by them; the amplitude of the arch indeed may be affected by ſuch deflections, if they happen at its extremities; but the effect of this error will be rendered the leſs, the greater the arch that is meaſured. We may ſuppoſe, therefore, that the chord of a large arch of a parallel of latitude is meaſured, and the amplitude of the arch itſelf at the ſame time accurately aſcertained. This laſt may be done, either by meaſuring the convergency of the meridians, if it be in a high latitude, or by any other method of aſcertaining differences of longitude which admits of great accuracy. The chord being thus given in fathoms, and the arch ſubtended by it being given in degrees and minutes, the radius of the parallel itſelf becomes known.

31. Now,

31. Now, if we would compare the radius of a parallel thus found, with a large arch of the meridian, we ſhall have by that means a determination of the figure of the earth, not leſs to be relied on than that given in the beginning of this paper. The inveſtigation is eaſy by help of the theorems in § 5 and 6. Let FO be the radius of a parallel to the equator, which paſſes through F, the latitude of which is ϕ, and is ſuppoſed known; and let FO found by the method juſt deſcribed be $= r$, then, as in § 4. $r = \frac{a^2 \operatorname{cof} \phi}{\sqrt{a^2 \operatorname{cof} \phi^2 + b^2 \operatorname{ſin} \phi^2}} = \frac{a \operatorname{cof} \phi^2}{a\sqrt{1 - \frac{2c}{a} \operatorname{ſin} \phi^2}}$, according to the method of reduction followed in the preceding articles of this paper. Then, becauſe $\sqrt{1 - \frac{2c}{a} \operatorname{ſin} \phi^2} = 1 + \frac{c}{a} \operatorname{ſin} \phi^2$ nearly, we have $r = a \operatorname{cof} \phi \left(1 + \frac{c}{a} \operatorname{ſin} \phi^2\right) = a \operatorname{cof} \phi + c \operatorname{ſin} \phi^2 \operatorname{cof} \phi$, or if we divide by $\operatorname{cof} \phi$, $\frac{r}{\operatorname{cof} \phi} = a + c \operatorname{ſin} \phi^2$. Let $\frac{r}{\operatorname{cof} \phi} = l$, then $l = a + c \operatorname{ſin} \phi^2$.

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32. Again, if ϕ' and ϕ'' are the latitudes of the extremities of an arch of the meridian, the length of which has been meaſured, and found $= l'$, then, according to § 5 we have

$$l' = a(\phi'' - \phi') - \frac{c}{2}\left((\phi'' - \phi') + \frac{3}{2}(\operatorname{ſin} 2\phi'' - \operatorname{ſin} 2\phi')\right).$$

If, therefore, m be the coefficient of a, in the former equation, and n the coefficient of c; and if m' be the coefficient of a, in the latter equation, and n' of c, we have, as in § 6.

$$a = \frac{n'l - nl'}{mn' - m'n}, \text{ and } c = \frac{m'l - ml'}{mn' - m'n}, \text{ or ſince } m = 1,$$

$$a = \frac{n'l - nl'}{n' - m'n}, \text{ and } c = \frac{m'l - l'}{n - m'n}; \text{ alſo } \frac{c}{a} = \frac{m'l - l'}{n'l - nl'}.$$

33. In this way of determining a and c, the parallel of latitude may either interſect the arch of the meridian meaſured or not. If it interſect that arch, this method may have the ſame advantage that was taken notice of in another ſolution, viz. that the whole of the *data* may be furniſhed from the ſame ſyſtem of trigonometrical operations. Thus, in the ſurvey of Great Britain, an arch of five or ſix degrees of a parallel to the equator might be meaſured, and compared with the whole length

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length of the meridian, comprehended between the northern and ſouthern extremities of the iſland, amounting nearly to nine degrees.

It is plain, from what has already been ſaid, that the reſult deduced from this compariſon would poſſeſs every advantage, and would be entitled to more credit, than any determination of the figure of the earth that is yet known.

34. On the ſuppoſition that, in a ſurvey of a country, the meaſurement is made along a ſeries of triangular planes, all given in poſition and magnitude, there is yet another method of determining the figure of the earth, more general than any of the former. On the ſuppoſition juſt mentioned, it is evident, that the length of a ſtraight line or chord, drawn from a given angle of any one of theſe triangles, to a given angle of any other of them, may be found by trigonometrical calculation. Let the latitudes be obſerved at the extremities of this chord, and alſo the difference of longitude; then, from the nature of an ellipſoid, the length of this ſame chord may be expreſſed, in terms of the axes a and b, together with the latitudes of the extremities of the chord, and the difference of longitude between them; and this expreſſion being put equal to the length of the chord meaſured, will give an equation, in which all the quantities are known, except a and b. Further, if $a = b + c$, and if the ſaid expreſſion be reduced into a ſeries, with the powers of c aſcending, that ſeries will converge very rapidly, becauſe c is ſmall in reſpect of a; then, for a firſt approximation, we may reject all the terms that involve the powers of c higher than the firſt, by which means we ſhall have a ſimple equation of the form $ma + nc = l$, where m and n are functions of the latitudes and difference of longitude, and l is the length of the chord.

Now, if a ſimilar equation be derived from the meaſurement of any other chord, theſe two equations will give a and c in the ſame manner as in § 6; and thus, from the meaſurement of any two chords, the figure of the earth will be determined.

35. The length of the chords, thus meaſured, ſhould be great, ſo that they may, if poſſible, ſubtend angles of ſeveral degrees, and their poſition will be moſt favourable when one of them is in the plane of the meridian, and the other nearly at right angles to it. The numerical computation will be found leſs laborious than might be imagined; but the complete

plete ſolution of the problem, and the full detail of the inveſtigation, I am under the neceſſity of delaying to ſome future communication.

To determine the earth's figure, by a general method with triangles.

There ſeems to be but one difficulty of any conſequence that ſtands in the way of this method of determining the figure of the earth. It ariſes from this, that the aſcertaining the poſition of the ſuppoſed ſeries of triangular planes relatively to one another, involves in it the allowance to be made for the terreſtrial refraction, which it muſt be confeſſed is not accurately known, and is the more difficult to determine, that it is unavoidably combined with the irregularities in the direction of gravity. It is poſſible, indeed, to ſeparate theſe two ſources of error, but not without a ſyſtem of experiments inſtituted directly for that purpoſe.

36. The determination of the difference of longitude, which enters neceſſarily into this problem, except in the caſe when both chords are in the direction of the meridian, muſt alſo be performed with great accuracy. Among the different ways of doing this, that which proceeds by obſerving the convergency of the meridians, though the beſt accommodated to the nature of a trigonometrical ſurvey, is not the leaſt liable to objection. For, not to mention that it is only practicable in high latitudes, we muſt obſerve, that it always implies a correction on account of the ellipticity of the meridian, which is therefore neceſſarily hypothetical, and depends on the very thing that is to be found. This inconvenience, however, may be obviated by repeated approximations, and by an accurate ſolution of *ſpheroidal* triangles. On this latter ſubject it was my intention to offer to the Society ſome theorems, that contain more direct and fuller rules for this kind of trigonometry than any that I have yet met with. I am under the neceſſity, however, of reſerving theſe, as well as the ſolution of the problem above-mentioned, for the ſubjects of ſome future communication. In the mean time, I think it is material to obſerve, that the principle laid down by Mr. Dalby, viz. that in a ſpheroidal triangle, of which the angle at the pole and the two ſides are given, the ſum of the angles at the baſe is the ſame as in a ſpherical triangle, having the ſame ſides, and the ſame vertical angle, is not ſtrictly true, unleſs the eccentricity of the ſpheroid be infinitely ſmall, or the triangle be very nearly iſoſceles. The application of the principle may therefore lead into error, unleſs it be made with due

attention to theſe reſtrictions. The gentleman, juſt named, will forgive a remark, which I certainly ſhould not have made, if I had been leſs intereſted for the ſucceſs of the work, in which he has aſſiſted with ſo much ability.

III.

Enquiries concerning the Nature of a Metallic Subſtance lately ſold in London as a new Metal, under the Title of Palladium: By RICHARD CHENEVIX, *Esq. F. R. S. and M. R. I. A.**

(Concluded from page 101.*)*

EXPERIMENTS TO PROVE AFFINITY AMONG THE METALS.

Inſtances of affinity in metals ſilver precip. by muriate of platina carried down 21 per. cent. of the metal.

Exp. 1. I diſſolved one hundred grains of ſilver in nitric acid, and precipitated by neutral muriate of platina. The precipitate, well waſhed and dried, was of a bright ſtraw-colour, and weighed 147 grs. Reduced in a charcoal crucible, it yielded a button weighing 121 grs. and of the ſpecific gravity of 11,6. The difference of weight, between the original hundred grains of ſilver and the 121, was owing to 21 grains of platina, which had been drawn down in precipitation along with the ſilver; by an affinity for that metal. This alloy is acted upon by nitric acid, and a great part of the platina is diſſolved along with the ſilver, nor is it very eaſy to ſeparate them by the common methods.

Mercury and ſilver precipitated in amalgam by green ſulphate.

Exp. 2. I diſſolved one hundred grains of ſilver in nitric acid, and added about 1200 of mercury. I poured the mixed ſolution into a ſolution of green ſulphate of iron, and obtained a very copious precipitate. When waſhed and dried, it weighed 939, and was a perfect amalgam, in the due proportion of mutual ſaturation. Its ſpecific gravity was 13,2; but no mercury remained with it after expoſure to heat.

Mercury and gold precipitated by green ſulphate gave a fine blue metallic powder.

Exp. 3. I diſſolved one hundred grains of gold in nitro-muriatic acid, and added to it about 1200 grains of mercury. Green ſulphate of iron, poured into this mixed ſolution, cauſed a precipitate weighing 874. It was in the form of a fine blue powder, not reſembling an amalgam, though wholly metallic. Its ſpecific giavity I could not aſcertain; but all the mercury was expelled by heat.

Experiments with recent muriate of tin, and

The re-agents which I uſed in the following experiments, were recent muriate of tin, and green ſulphate of iron. To

bring the examples of anomalous precipitations, in mixed solutions of the metals, more clearly into view, it will be necessary to state the action of these salts, upon a solution of each metal when separate.

green sulphate of iron, as re-agents.

By recent muriate of tin we have, with a solution of gold, the well known purple of Cassius. With platina, the colour of the liquor is much heightened. With mercury, there is a total reduction. With copper, a reduction from the black oxide at 20 *per cent.* of oxigen, to the yellow oxide at 11,5 *per cent.* of oxigen. With arsenic acid, a reduction to the state of white oxide. With silver, with lead, with antimony, no reduction. Green sulphate of iron reduces none of the metallic solutions, except those of gold and silver.

Action of the recent muriate of tin on solutions of gold, platina, mercury, copper, arsenic acid, silver, lead, antimony.

of green sulpbate of iron, reduction of gold and silver only.

When mixed solutions of the metals are exposed to the action of recent muriate of tin, or of green sulphate of iron, we have the following results.

Experiments 4, 5, 6, 7, and 8. Muriate of tin, poured into a mixed solution of gold and mercury, precipitates both metals together; and there are no traces of the purple. Mixed solutions of gold and antimony, also of gold and arsenic acid, are acted upon in the same manner. Mixed solutions of gold and copper, also of gold and lead, afford results similar to those of each metal when separate.

Exp. with mixed solutions. Muriate of tin the re-agent.

Experiments 9, 10, 11, 12, and 13. With a solution of platina and arsenic acid, muriate of tin gives no precipitate; but the colour of the liquor is more heightened than if the platina had been alone in solution. Platina and antimony give a precipitate by this re-agent, after standing some time; but the effect is retarded by the excess of acid in the solution of antimony. Platina and copper, also platina and lead, are acted upon as the separate solutions of these metals. Platina and silver are precipitated together by green sulphate of iron.

Green sulphate

Experiments 14, 15, 16. Mercury and copper, mercury and lead, also mercury and arsenic, are precipitated in the metallic state by recent muriate of tin.

From these experiments it is evident,

1st. That gold has an affinity for mercury, for antimony, and for arsenic.

Deductions of affinity between gold and mercury, antimony, arsenic; platina and silver, mercury, antimony; silver and mercury; mercury and copper, lead, arsenic.

2d. That platina has an affinity for silver, for mercury, and for antimony; and that it is influenced by the presence of arsenic.

3d. That silver has an affinity for mercury.

4th. That mercury has an affinity for copper, for lead, and for arſenic.

This ſeries of experiments is not intended as a ſyſtem of metallic affinities; but as a few facts ſtated to corroborate an aſſertion. I am well aware that many others might be noticed; but it is not my intention to enter further into this ſubject, in the preſent paper. The general importance of the principle, and the extenſive influence it is likely to have upon chemiſtry, demand that it ſhould be treated by multiplied reſearches. The experiments that can elucidate it are of the moſt delicate nature, and require peculiar care; for they do not always ſucceed, unleſs performed under the moſt favourable circumſtances.

Mixed ſolutions of more than two metals are more ſtriking and more complex.

When mixed ſolutions of three or more metals are expoſed to the action of recent muriate of tin, or of green ſulphate of iron, their action upon each other appears in a much more ſtriking, as alſo in a much more complicated point of view.

EXPERIMENTS UPON PLATINA.

Exp. on platina.

I ſhall now ſtate ſome experiments which I have had occaſion to make upon platina, during the foregoing reſearches. Very little is known concerning this metal, its oxides, or its ſalts; and, although I have not had occaſion to extend the enquiries very far, yet my experiments may ſerve to eſtabliſh a few points.

Platina diſſolved precip. by lime; rediſſolved in nitric acid; the acid expelled from the oxide by heat; oxide reduced by ſimple ignition.

I diſſolved a quantity of purified platina * in nitro-muriatic acid, and precipitated by lime. A great portion of platina remained in the liquor, although I had uſed an exceſs of the above earth. I rediſſolved the precipitate in nitric acid, and evaporated to dryneſs. The reſult was, a ſubnitrate of platina. I then expoſed the maſs, in a crucible, to a heat capable of expelling the acid altogether; and the oxide remained alone. When this was reddened, at a heat which certainly was not capable of melting ſilver, the oxide was reduced, and appeared with a metallic luſtre. The weight of the various

* By purified platina, I have always underſtood, in this Paper, platina reduced, at a gentle heat, from the ſalt obtained by pouring a concentrate ſolution of muriate of ammonia into a concentrate ſolution of platina.

products

products, in the above experiments, was such as to give the following proportions in the oxide, and the subnitrate of platina.

Hence component parts of the oxide;

Yellow oxide of platina is composed of,

Platina - - -	87
Oxigen - - - -	13
	100.

and of the subnitrate.

Subnitrate of platina is composed of,

The above oxide of platina -	89
Nitric acid and water - - -	11
	100.

Two oxides of platina; yellow with 13 per. cent. oxygen; green with only 7 per. cent.

But, in the reduction of this oxide of platina, it became of a green colour; and remained during some time in that state. Nitrate of platina sometimes becomes of a pale green at the edges, when evaporated to dryness; and ammonia assumes a green colour when in holds oxide of platina in solution, as we have seen more particularly with palladium. This, therefore, is a second oxide of platina. It contains but seven *per cent.* of oxigen.

I dissolved a known portion of platina in nitro-muriatic acid, and expelled the nitric acid, by pouring in a sufficient quantity of the muriatic; and then evaporated to dryness. By this experiment I learned, that the insoluble muriate of platina is composed of,

Compon. parts of insoluble muriate of platina;

Yellow oxide of platina - -	70
Muriatic acid and water - - -	30
	100.

I then expelled the muriatic acid by the sulphuric, and evaporated again to dryness. I found the insoluble sulphate of platina to be composed of,

and of the insoluble sulphate.

Oxide of platina - - -	54,5
Acid and water - - - -	45,5
	100,0.

Muriate of tin is the most delicate test of platina.

By much the most delicate test for platina is muriate of tin. A solution of the former, so pale as hardly to be distinguished from water, assumes a bright red by a single drop of the recent muriatic solution of the latter metal. If mercury be present, the colour is much darker. Recent muriate of tin, poured into a solution of the muriate formed by the red oxide of mercury,

converts it into the muriate formed by the leſs oxigenized acids; but, ſhortly after, the mercury is reduced to the metallic ſtate. Hence it was, that the alloy of platina and mercury always gave a deeper coloured precipitate than platina, with muriate of tin.

The pruſſiates do not precip. platina or mercury ſingly; but they do jointly.

Neither platina nor mercury are precipitated by pruſſic acid or by the pruſſiates. But, if ſulphate, nitrate, or muriate of platina be poured into pruſſiate of mercury, an orange-coloured precipitate is immediately formed; and, in ſome caſes, a mixed ſolution of platina and mercury gives a ſimilar precipitate by pruſſic acid alone.

Platina is ſimply precip. by ſulph. hidrogen.

Platina is one of the metals which are precipitated by ſulphuretted hydrogen, without the neceſſity of a double affinity.

Order of affinities of platina with acids.

The affinities of platina differ much from what is generally ſtated in the tables. By the few acids I have had occaſion to try, oxide of platina is attracted in the following order: ſulphuric, oxalic, muriatic, phoſphoric, fluoric, arſenic, tartaric, citric, benzoic, nitric, acetic, and boracic.

Remarkable theoretical obſervation.

That ſulphuric acid ſhould attract the oxide of platina with greater force than the muriatic, is an unanſwerable argument to an opinion which was long ſupported by many philoſophers, and which is not yet altogether abandoned by them. Muriatic acid has been ſaid to contribute to the ſolution of gold or platina, in nitro-muriatic acid, in the ſame manner as ſulphuric acid is ſuppoſed to promote the decompoſition of water, during the ſolution of iron by that acid diluted. The affinity of muriatic acid for the oxide of gold or of platina, has been looked upon as the diſpoſing cauſe that nitric acid is decompoſed by thoſe metals. But it is evident that ſome other action takes place; for, ſulphuric acid, which has a ſtronger affinity for oxide of platina than muriatic acid, does not in the leaſt promote the decompoſition of nitric acid by gold, or by platina.

CONCLUSION.

On the nature and uſe of general hypotheſes.

The ſubſtance which has been treated of in this paper, muſt convince us how dangerous it is to form a theory before we are provided with a ſufficient number of facts, or to ſubſtitute the reſults of a few obſervations, for the general laws of nature. If a theory is ſometimes uſeful, as a ſtandard to which we may refer our knowledge, it is at other times prejudicial, by creating

ing an attachment in our minds to preconceived ideas, which have been admitted without inquiring whether from truth or from convenience. We eaſily correct our judgment as to facts; and the evidence of experiment is equally convincing to all perſons. But theories, not admitting of mathematical demonſtration, and being but the interpretation of a ſeries of facts, are the creatures of opinion, and are governed by the various impreſſions made upon every individual. Nature laughs at our ſpeculations; and though from time to time we receive ſuch warnings as ſhould awaken us to a due ſenſe of our limited knowledge, we are preſented with an ample compenſation, in the extenſion of our views, and a nearer approach to immutable truth.

The affinities of metals for each other are likely to be of the moſt extenſive influence in chemiſtry. They will promote ſcepticiſm with regard to future diſcoveries, and throw ſome doubts upon our preſent knowledge. Palladium is certainly not leſs different from the elements that compoſe it, and from all other metals, than any two can be from each other. Within the laſt fifteen or twenty years, ſeveral new metals and new earths have been made known to the world. The names that ſupport theſe diſcoveries are reſpectable, and the experiments deciſive. If we do not give our aſſent to them, no ſingle propoſition in chemiſtry can for a moment ſtand. But, whether all theſe are really ſimple ſubſtances, or compounds not yet reſolved into their elements, is what the authors themſelves cannot poſitively aſſert; nor would it in the leaſt diminiſh the merit of their obſervations, if future experiments ſhould prove them to have been miſtaken, as to the ſimplicity of thoſe ſubſtances. This remark ſhould not be confined to later diſcoveries; it may as juſtly be applied to thoſe earths and metals with which we have been long acquainted.

The affinity of metals for each other, ſo ſtrongly ſhewn in the preſent inſtance, may induce doubts to the nature of other new metals;

With regard to the metals, we have ſeen how little dependance is to be placed on ſpecific gravities. A contrary anomaly to that which operates upon platina and mercury, may take place in others; and they may become as much heavier than the mean, as the former become lighter. In this ſtate of union, they may for a long time appear homogeneous, even by the teſt of chemical re-agents. One of the properties that renders metallic ſubſtances ſo precious is, their eaſy formation into ſuch inſtruments as our neceſſities require. The fragile metals are but of ſecondary conſequence; and, at moſt, ſerve to con-

Brittle metals may perhaps be ſtill more ſimplified.

on thoſe which are ductile, ſome quality which adapts them better to particular purpoſes. It often happens that, by being alloyed, two ductile metals become fragile; but we have no inſtance of the contrary effect in any high degree. It is therefore more to be ſuppoſed that we ſhould look to ſimplification among the fragile metals; and, even at this early period, it may not be too ſpeculative to conſider the metallic bodies in an order which may bring together thoſe which poſſeſs the greateſt number of ſimilar characters.

Compariſons of metallic bodies with this view;

As an inſtance of this approximation, it may be obſerved, that nickel and cobalt ſtrongly participate in the properties of copper and iron. The two former metals were long regarded as mixtures; and the doubts of the ancient chemiſts, who feared to pronounce as to their nature, may ſtill be proved to have more foundation in truth than the aſſertion of the moderns, who have declared them to be ſimple. Acted upon by the ſame menſtrua, forming inſoluble compounds with the ſame acids, and ſoluble alike in other ſubſtances, they have but one or two marked properties that lead us to conſider them as diſtinct metals. But palladium has at leaſt five or ſix characters, as ſtrong as thoſe of any metal whatſoever, that diſtinguiſh it, not only from its elements, but alſo from all other metals.

and earths alſo.

Among the earths, this approximation is ſtill more apparent. A leading character of theſe ſubſtances is, their tendency to enter into ſaline combination, in which they receive new properties, and perform new functions. If we rank them according to this general tendency, we ſhall have the following order: barytes and ſtrontia; lime and magneſia; glucine and alumina; zircon and ſilica. And, if we conſider them two by two in this order, which is a natural one, we ſhall bring together preciſely thoſe which differ by the ſmalleſt number of chemical characters.

Concluſion.

This inveſtigation might be purſued ſtill further; but we muſt wait the reſult of experiments: a wide field is open for reſearch. In the dark ages of chemiſtry, the object was, to rival nature; and the ſubſtance which the adepts of thoſe days were buſied to create, was univerſally allowed to be ſimple. In a more enlightened period, we have extended our enquiries, and multiplied the number of the elements. The laſt taſk will be to ſimplify; and, by a cloſer obſervation of nature, to learn from what a ſmall ſtore of primitive materials, all that we behold and wonder at was created.

IV. *Obſervation*

IV.

Obſervations on ſome remarkable Strata of Flint in a Chalk-Pit in the Iſle of Wight, in a Letter from Sir HENRY CHARLES ENGLEFIELD, *Bart. F. R. S. to* JOHN LATHAM, *M. D. F. R. S. and L. S.* *

DEAR SIR,

Introductory obſervations.

AS you conſidered the ſpecimens of flint which I ſhewed you worthy of the notice of the Linnean Society, I tranſmit them to you, together with ſuch an account of the ſituation in which I found them, as may perhaps lead to a gueſs of the cauſes of their preſent very extraordinary condition, and will at leaſt ſerve as a guide to thoſe who may wiſh at a future time to inſpect the curious pit where I found them.

Before I enter on the particular deſcription of that ſpot, I cannot help ſaying a few words on the lithology of the iſland in general, which has not that I know of, been deſcribed, as it highly deſerves, by any naturaliſt. Had I been equal to ſuch a taſk, opportunities of obſervation were wanting, and the phenomenon which I am about to deſcribe, was diſcovered by me ſo ſhort a time before I quitted the iſland, that I had not time to inſpect more than one pit, beſides that in which I obſerved it.

Geological account of the Iſle of Wight.

The Iſle of Wight, which is nearly of a rhomboidal form, lies with reſpect to its four angles, almoſt abſolutely in the four points of the compaſs. It is divided into two very nearly equal parts by a range of chalk hills, whoſe general direction is due eaſt and weſt. Theſe hills do not, however, lie in a ſtraight line, nor are they at all of equal breadth or height throughout their extent. At Bembridge, where they form the eaſtern point of the iſland, they riſe abruptly from the ſea to the height of about 400 feet; and bending a little to the northward, they continue of nearly the ſame elevation, and a very narrow breadth, till they terminate at the valley through which the Medina runs. To the weſt of the Medina the range grows conſiderably wider and is ſubdivided into ſeveral ſubordinate vallies. This additional breadth gives the ſouthern limit a great curvature to the ſouth, while the northern line remains nearly ſtraight. Their elevation increaſes much, and at Mottiſton is 700 feet. The acute and perpendicular

* From the Tranſ. of the Linnean Society, vol. IV.

promontory

Geological account of the Iſle of Wight.

promontory in which they terminate to the weſt, well known by the name of the Needles is nearly as high as Mottiſton. Beſide the valley of the Medina, this range is ſingularly interrupted by two vallies exactly ſimilar to each other, at the two ends of the iſland. Brading Haven renders Yaverland at the eaſt almoſt an iſle, and the Yarmouth inlet cuts off the weſtern end ſo nearly, that at high tides it is quite inſulated at freſhwater gate.

To the north of this range of chalk hills the ſoil is chiefly clay, with a ſuperſtratum, in many parts of gravel, the clay is interſected with many beds of ſtone of different qualities, and which appear to lie in great confuſion. Of theſe ſome are grit with a ſlight admixture of calcareous matter, others have nearly equal parts of ſand and lime, and others are purely calcareous. In the firſt which are of great hardneſs, very few extraneous bodies appear. In the ſecond, are many fine impreſſions of ſhells, while the laſt are almoſt entirely compoſed of moulds of turbinated ſhells, ſo as to appear quite honey-combed by them. This ſtone is however, of great durability for the walls of Cowes Caſtle, which was built by Henry VIII. and is expoſed to the ſea air from the weſt and north, are as perfect as on the day in which they were built. Below all theſe ſtrata of ſtone at eaſt Cowes, and juſt above a bed of black and ſolid clay is a ſtratum of ſhells about two feet thick, of which a ſpecimen accompanies this; and which is totally compoſed of theſe ſhells without any admixture or earth whatſoever. As the Lea makes great inroads here, vaſt heaps of theſe ſhells lie on the beach, and ſeem juſt waſhed up by the waves, inſtead of being torn from their bed in the cliff. They appear nearly in the ſame ſtate as thoſe on the Hampſhire coaſt, which have long been famous among naturaliſts. In the bed at eaſt Cowes, there appears, however, no variety; for I could ſee no ſpecies but what are here exhibited.

Whatever confuſion in the ſtrata appears to the north of the chalk range, or in that range itſelf, diſappears to the ſouth of it, where the ſtrata are nearly in a horizontal poſition, and ſingularly regular and undiſturbed. The ſea coaſt from Bembridge, ſouth to the Needles, except in the ſmall extent of Sandown Marſh, is every where higher than the immediately contiguous land of the iſland, and to the ſouth-eaſt, riſes into

a

a vaſt range of hills running from Dunnoſe weſt to St. Catharines. The ſubſtratum of theſe hills ſeem every where to be clay lying in ſtrata of different colour and purity. The loweſt is black and very hard; approaching to ſhale. Above this ſome ſtrata have a great mixture of ſand, and take the appearance of a ſoft ſtone, breaking into very regular cubical forms. Theſe ſtrata extend over the whole ſouthern part of the iſland, and terminate againſt the chalk range very ſuddenly. Above the clay ſtrata is a bed of ſtone in thin layers, and of very mingled materials, but in general very hard. Great quantities of chert or flint modules appear in this ſtone. The general thickneſs of the ſtratum is from 150 to 200 feet. Above this the higheſt hills of the range have a ſtratum of chalk, not pure or white as that of the chalk range properly ſo called, nor producing flint ſo black. Geological account of the Iſle of Wight.

The height of Dunnoſe is 800 feet above low water mark, St. Catharine's hill is at leaſt 850. Of the former I had no opportunity of examining accurately the thickneſs of the ſtrata, but at St. Catharine's the ſtrata are as follow,

Chalk	-	250 feet
Stone	-	200 feet, or perhaps not quite ſo much
Clay and Sand	-	400 feet.
		850.

This arrangement accounts entirely for the formation of that ſingular coaſt called the Undercliff, which extends from Dunnoſe to St. Catharine's; and is compoſed of the confuſed fragments of the upper ſtratum of rock, which have given way and rolled down, as the ſubſtratum of clay has been waſhed away by the ſea. In moſt parts the proceſs ſeems nearly at a ſtand; the coaſt being now protected by the fallen rocks; but at St. Catharine's, great devaſtation is ſtill taking place. The earth-fall mentioned laſt year, was a very ſmall operation, when compared with the relics of former convulſions.

From this ſhort ſketch of the general poſition of the ſtrata in the iſland, I return to the particular ſubject of the preſent paper.

The chalk pit, which I am about to deſcribe is ſituated on the northern edge of the chalk range juſt out of the village of Cariſbrook, and about an hundred yards beyond the diviſion of the roads to Yarmouth and Shorwell. The pit is open to the Deſcription of a chalk pit at Cariſbrook, with ſeams of flint, ſingularly broken or ſhattered.

the eaſt. The ſtrata of chalk are very regular from two to five feet in thickneſs, and divided by ſeams of flint from ſix inches to nine inches in depth: The flints are as uſual in nodules of different ſizes from the ſize of the fiſt to twice the ſize of a man's head. The whole dip northward with an inclination of at leaſt 67 degrees. Perpendicular fiſſures run through the whole from north to ſouth, the ſides of which are nearly as flat and ſmooth as a wall. As theſe fiſſures are followed with convenience in working the pit, an extenſive face was laid open when I ſaw it, and the appearance was as in the annexed ſketch. See *Plate* XI. On examining the beds of flint nearly, I was aſtoniſhed to find that every flint, though lying in its place, and retaining perfectly its original ſhape, was more or leſs burſt and ſhattered, ſome few were only ſplit into large pieces, but the greater part were broken into ſmall fragments, and ſome abſolutely reduced to impalpable powder. From one which had ſuffered the moſt, the annexed ſpecimen was taken. The powder was ſo very fine that I conceived it muſt have been mixed with chalk; but on waſhing it with diluted marine acid, I found that it was purely ſiliceous. Indeed the chalk which ſurrounds theſe flints is uncommonly ſolid, and does not exhibit cracks or marks of any violence except the great fiſſures before mentioned. A ſpecimen of the flint powder after waſhing in the acid is ſent with the other.

I muſt obſerve that I had but an imperfect opportunity of inſpecting the flints which lay at a diſtance from the fiſſure, ſuch however as I could ſee in the bed then working appeared to have been leſs ſhattered in proportion as they were more remote from the fiſſure, but all had ſuffered more or leſs.

About 200 yards below the pit, and nearer to Cariſbrook village, the road is in part cut through the chalk, and the beds of flint expoſed by that means exhibit the ſame appearances as thoſe in the pit above.

Obſervations of the flints in another pit.

The chalk pit above Shide Bridge, which is the only one I had an opportunity of examining after my diſcovery of the phœnomenon above deſcribed, preſents in ſome degree the ſame appearances, but does not afford ſo good an opportunity of viewing the ſtrata as that at Cariſbrook. The ſtrata did not appear to me to lie ſo regularly nor the flints to be diſpoſed ſo much in beds as at Cariſbrook. They were however extremely

tremely broken and ſhattered and apparently the moſt ſo where they lay moſt in ſtrata. The ſtrata had alſo a great inclination or dip to the north.

Although it would be raſh to attempt to account for this very ſingular ſtate of deſtruction of the flints in the Cariſbrook pit, yet it is impoſſible not to offer ſome conjectures on the ſubject. There can be very little doubt that the ſtrata though now ſo inclined was originally formed in a horizontal poſition. When the tremendous convulſion took place which ſunk them to the ſituation in which they now appear (at which time the channel which ſeparates the Iſle of Wight from the main land was perhaps formed) the ſtrata of chalk, in the act of ſubſidence had a tendency to ſlide on each other, and this would be exerted moſt ſenſibly where from the admixture of the flints the coheſion of the parts of the chalk was the weakeſt. This motion or rather ſtrain of ſo enormous a weight might in an inſtant ſhiver the flints, though their reſiſtance ſtopped the incipient motion, for the flints though cruſhed to powder are not diſplaced, which muſt have been the caſe had the beds ſlipped ſenſibly. This conjecture is perhaps ſtrengthened by what I obſerved in a few detached nodules of flint in the chalk ſtrata which did not appear to have ſuffered as thoſe in the beds of flint have done. I may here add that it ſeemed as if in ſome places the fine powder of the flints had run down, and inveſted the nearer parts of the fiſſure with a thin coating of the agglutinated duſt; but this may poſſibly have taken place ſince the face of the fiſſure has been expoſed to the weather.

Conjecture as to the cauſe; viz. that the ſtrata at the period when their dip took place were inclined to ſlide on each other, and the flints were cruſhed by this enormous action.

Perhaps it may not be totally foreign from the preſent ſubject to mention that in a very great chalk pit at the village of Preſton, a mile north of Brighthelmſtone, in which the flints lie in a very regular and nearly horizontal ſtrata, but which has alſo vaſt perpendicular fiſſures in the chalk; the fiſſures are in many places filled to a conſiderable extent with a very thin vein of pure flint exactly as if the flint, not being quite hard when the fiſſures took place, had been ſqueezed out of the beds and run into the fiſſures as ſoft pitch would do. I do not mean at all to ſay that this was the caſe, but merely to deſcribe the appearances. In the chalk pit juſt below the church at Brighthelmſtone another ſingular appearance may be ſeen. The upper part of the chalk is in ſeparate maſſes, not properly rubble, but with all their tender angles ſharp exactly as if juſt broken

Appearances of a chalk pit near Brighthelmſtone.

broken to pieces to put into the lime kiln, and quite clean, nearly of a ſize, and almoſt without any chalk powder mixed with them.

I remain, &c.

Southampton, Jan. 22, 1800.

V.

Deſcription of a mercurial Air-pump, of unlimited exhauſting Power, with a Wooden Piſton, working in a Wooden Cylinder, &c. By Sir A. N. Edelcrantz, *&c. &c. Communicated by the Author.*

Conſtruction of an air pump in which no reſidual ſpace is left.

HAVING reſided a long time in a place, where no mathematical inſtrument maker could be had, I was obliged to contrive ſome method of conſtructing an air-pump, in which common workmen and common materials could be employed. Beſides which, as in almoſt all former conſtructions of this inſtrument, a ſmall ſpace exiſts in ſome part of them, where the air remains condenſed, and conſequently a limit is fixed to the expanſion, I was deſirous, if poſſible, to remove this ſeemingly material, though in good pumps infinitely ſmall defect. Theſe conſiderations led me many years ago to a contrivance, of which the following ſhort deſcription may give a notion, reſerving a more detailed one, with a more particular account of its effects, to a future publication.

Deſcription with a drawing. It operates by mercury raiſed and lowered in a wooden pipe, by a plug, and the upper communication with the receiver is made through an iron cock.

A B C D, *Plate* XII. is a ſolid piſton of wood, cylindrical or ſquare, * moving up and down in a ſimilar wooden box or tube, E F G H, by means of rack-work at H. The piſton fills the box in its whole length as exactly as poſſible, without touching and producing friction againſt the ſides. For that purpoſe the piſton is guided at the lower end by an iron rod K I, fixed in the bottom of the box and entering the piſton, and at its upper part by a roller, which runs along the inner ſurface of the barrel. The cavity of the wooden box extends through the canal F M† to the iron cock O, and communicates at M with a per-

* I have, in the execution, preferred the ſquare form, as more eaſy to make exact and equal in its whole length.

† All the wooden parts are made of ſtrong oak, and are covered with a good reſinous varniſh, to prevent the abſorption of mercury:

pendicular

pendicular glaſs tube M N and the glaſs ſphere P. This ſphere is connected with an iron cock Q R, having two oppoſite perforations, one going horizontally to R, where a ſmall outſide valve permits a communication with the glaſs ball S Y; the other, placed perpendicularly, opens a paſſage from the common receiver V T to the ſphere. The inſtrument being thus conſtructed, the box H E F G, is filled with mercury to the dotted line *a a*, or about ſix inches, and the piſton being alternately preſſed down or raiſed as uſual by the crank work, the effect of the machine is as follows:

Air pump operating by mercury in a wooden barrel, &c.

The cock Q R being turned as in the figure, the communication with the receiver is ſhut, and that with the external air, through a ſmall hole S in the glaſs ſphere S Y, being open, the piſton in deſcending preſſes upon the ſurface *a a*, and forces the mercury out of its place, it conſequently riſes in the ſmall ſpace between the piſton and the wooden tube, *a a* to *b b*, and at the ſame time in the glaſs tube N P to a correſponding height N. The preſſure being continued, the mercury ſtill riſes ſo as to fill the whole tube, the ſphere P, and laſt of all makes its way through the cock Q R and the valve R. forcing every particle of the air contained in M N P out before it. When a drop of mercury appears in Y, the piſton is moved the contrary way, and by its aſcenſion the preſſure on the mercury is diminiſhed, which ſinks and evacuates the ſphere P, leaving a place for the air from the receiver which is admitted by turning the cock Q R, in the other direction *. P being filled with air; the cock is turned again, the piſton again deſcends, and expels the air, by raiſing the mercury a ſecond time, and in this manner the exhauſtion may be continued at pleaſure: The mechaniſm and effect of this inſtrument being, as I preſume, rendered clear by the preceding deſcription, I ſhall only add a few remarks.

The neceſſary force to preſs down the piſton, being a little more than the whole weight of a column of mercury of the baſis D C, and the height of the barometer, is conſiderable in the beginning of the operation, but diminiſhes as in other air-pumps, when the air in the receiver becomes more rarefied; when the external atmoſphere acts more powerfully in ſupport

* The alternate turnings of the cock may eaſily be performed by a ſimple appendage connected with the motion of the piſton.

Air pump operating by mercury in a wooden barrel, &c.

of the column M N, which in every ſucceſſive ſtroke becomes higher, till the exhauſtion is nearly compleat, when its height will be that of the barometer.

In order to equalize the preſſing force more nearly during the whole operation, the upper end of the piſton may be loaded with a weight equal to half the original preſſure.

The mercury being a fluid, fills every angular ſpace as paſſes, till it opens its way through the valve R; conſequently no particle of air can be left in a condenſed ſtate to be more rarified or diminiſhed by the next ſtroke of the piſton. By this means the common defect of air-pumps ſeems to be remedied, and the exhauſtion unlimited. Beſides, as neither moiſture or oil are introduced in this pump, the vapours ariſing from them in great exhauſtions are prevented.

The quantity of mercury required, when the diameter of the wooden box and of the ſphere P are four inches, will be about ſixty pounds, which if taken out by the cock O, may ſerve alſo for another uſeful apparatus in the ſame laboratory: the mercurial air holder.

Several projects for mercurial air-pumps have been publiſhed before, although I think none executed; but a compariſon will, as I ſuppoſe, eaſily ſhew this conſtruction to be quite different from any of them.

For greater ſolidity, I have in the execution made three-fourths of the tube M P of iron. The ſphere P is likewiſe fixed and faſtened by means not ſhewn in the figure, but which may without difficulty be apprehended.

Excepting the cock Q R, which requires ſome preciſion of workmanſhip, all the parts of the machine may be executed by common workmen, ſuch as carpenters, ſmiths, &c. who are every where to be found.

VI. *On*

VI.

On the Combinations for ſupplying Worm Tubs with Cold Water, by the Syphon, with an eaſy Method of ſecuring the Joints of ſuch Hydraulic Apparatus againſt the Admiſſion of the External Air. In a Letter from Mr. WM. CLOSE.

To Mr. NICHOLSON.

SIR,

Dalton, Feb. 13, 1804.

Reaſons why the ſyphon principle is applicable to the ſupply of worm tubs, &c.

THE combinations which have lately appeared in your Journal, for ſupplying worm tubs, condenſers, &c. with cold water, have recalled my attention to certain experiments which I formerly made with the ſyphon; and I am of opinion that the method propoſed will ſucceed very well upon the ſimple conſtruction of the apparatus delineated in *Plate* IV. *Vol.* VI. of your Journal, if all the parts are made air-tight, and a ſufficient quantity of water be provided to keep the ſyphon in conſtant action, under an impending column of two feet. For the air diſengaged from the water in the worm-tub will riſe into the bend of the ſyphon at B. *Plate* IV. *Vol.* VI. and be carried down the deſcending column and emitted at C.

Eaſy method of excluding the external air, by ſurrounding the worm tub with water.

The worm tub may be effectually ſecured againſt the admiſſion of the external air through the joinings of the ſtaves, by being placed in a larger tub filled with water; no air then can poſſibly enter the interior tub ſo long as it is covered, and the workmanſhip muſt be very bad, if the ſyphon ſhould draw the water out of the exterior veſſel, ſo faſt as to produce any inconvenience in repleniſhing it as often as it ſubſides.

To ſet the machine at work when ſtopped.

The worm muſt be made air-tight before it is fixed in its place. If the ſupply of cold water at A ſhould be interrupted, the fluid in A will ſubſide to a level with the ſurface of C, and, if much air be diſengaged during this ſuſpenſion, and while the water is impending in the apparatus, the ſyphon will probably not reſume its operation: In this caſe the two apertures muſt be cloſed, and the apparatus filled with water.

Account of the effect of a ſyphon engine formerly deſcribed.

You will remember the principles on which I planned the conſtruction of a machine for raiſing water by the ſyphon. I have the complete model of ſuch a machine: it has four light, ſelf-acting valves, and when theſe are not prevented from cloſing, by impurities in the water, &c. it performs very well.

The

The ſyphon is not quite two inches wide; it riſes about 34 inches above the reſervoir in which it is placed, and the deſcending column is ſix inches longer than the aſcending one. The machine is conſtructed with all the parts repreſented in *Plate* III. *Vol.* I. but under a different arrangement. It delivers about three pints of water every minute, at 31 inches above the reſervoir: the alternations are repeated every five ſeconds.

Its power in carrying down air.

In the operation of this machine, a bulk of air equal to three pints of water is carried down the deſcending column every minute, when the fall is only ſix inches; but I am certain from experiment that the ſyphon would clear itſelf of a much greater bulk in the ſame time.

Hence the worm tub ſyphon will be effectual in carrying off the air extricated from the water.

In an experiment on the 29th of May 1802, with a ſyphon of the above dimenſions, and a fall of ſix inches, it appeared by a graduated bottle which received the raiſed water, that the ſyphon would take a quantity of air equal in bulk to four ounces of water at leaſt, every three ſeconds. I think, therefore, that the ſmall quantity of air which will be diſengaged from the water in the worm tub at a low temperature, by the operation of the ſyphon, with a fall of two or three feet, will be carried off as faſt as it is produced, unleſs the pipe be very narrow in proportion to the tub,

I am, Sir,

Your's reſpectfully,

WILLIAM CLOSE.

VII.

An Anſwer to Dr. WOLLASTON's *Statement, "Of an Improvement in the Form of Spectacle-Glaſſes." By Mr.* WILLIAM JONES, *F. Am. P. S. Optician, Holborn.*

The ſpectacle glaſſes of Dr. Woollaſton cenſured.

OBSERVING in your Journal for laſt Month, that Dr. W. H. Wollaſton, by a paper inſerted therein, is attempting to introduce into the conſtruction of ſpectacles, the well known and obſolete form of lens called *a meniſcus*; I beg leave, Sir, to offer through the channel of your ſcientific and impartial record, a few obſervations on his arguments; and my reaſons why I do not conſider the contrivance as entitled to any claim either of novelty or improvement.

When

When a printed book, or other object, is received through a convex ſpectacle-glaſs, or other lens of a ſhort focal diſtance, ſuch as from ſeven inches down to four inches or leſs, the indiſtinctneſs obſerved of the ſurrounding parts, when the central appear diſtinct, ariſes from the ſpherical figure of the lens, and is by Opticians called the longitudinal aberration of the lens. There is another kind of aberration connected with this lens, called the lateral aberration, which is occaſioned by the priſmatic form of the lens, producing different refrangibility of the rays of light, and blending the priſmatic colours with the appearance of the object. It is the longitudinal aberration only that I have now occaſion to conſider. This aberration in lenſes of the ſame foci, increaſes with the diameter and thickneſs; and of the ſame diameter is in the inverſe proportion to the foci. Aberration of light refracted by lenſes.

The rays iſſuing from diſtant objects, are more parallel to each other when incident upon the lens, than thoſe from proximate ones; therefore the aberration will be leſs.

Hence, it may be inferred, that when ſpectacle-glaſſes have been made larger in diameter than is ſufficient to obſerve through, the angular extent of objects, they refract ſuperfluous light, and have very properly been ſomewhat reduced in diameter, ſo that a perſon might uſe them without much inclining the axis of his eyes, or finding it inconvenient or unnatural to move his head a little. For the aberration is thus diminiſhed, and conſequently the objection in a great degree removed, except when the glaſſes are of very ſhort foci. Contracting the diameter of ſpectacle lenſes approved.

In concave glaſſes, the aberration or indiſtinctneſs is of a ſimilar nature; the defect of theſe being from the imperfect divergence of the rays, inſtead of the imperfect convergence in convex glaſſes. Concaves.

Spectacle-glaſſes are now generally made of the double concave, and double convex forms or nearly ſo; for a little alteration of figure does not affect the general appearance of objects viewed through them. It is in ſcience, as in other caſes, that general utility does not always depend on trifling alterations. Spectacles are recorded to have been invented about the year 1300, and from much reading and many years experience in this ſmall, but invaluable article, I really do not know, that during the elapſed time, any optical inſtrument of any kind whatſoever has undergone more innovation, and Remarks on ſpectacles and ſome of the improvements made in them.

attempts at improvement. Of many within my knowledge, I ſhall only ſelect the following as entitled to any degree of commendation. *Ayſcough's* crown glaſs ſpectacles. The biſected glaſſes of *Dr. Franklin*. *Ribright's* double glaſſes. The viſual glaſſes of that learned Optician *Mr. Martin*. The ſquare convex form by *Stour*. The patent combined glaſſes of Meſſrs. Watkins and Smith, injudiciouſly called achromatic, conſiſting of a convex lens, combined with a *meniſcus* or concave convex lens. In the various mountings of the frames there is a ſtill greater variety.

The original form moſt approved.

Notwithſtanding theſe contrivances, univerſal experience has cauſed the original and ſimple form of glaſſes to ſuperſede them; and it affords an indubitable proof, that it is the beſt and moſt convenient that can be deviſed, when clear glaſs, accurate tools, and good workmanſhip are uſed.

Theorem of Huygens conſidered as univerſally applicable.

The theorem given by *Huygens*, and demonſtrated by many other ſubſequent writers on optics, proving that a convex lens having its radii of curvature in the proportion of one to ſix, has leſs aberration than any other form of lens, when the greateſt convexity is towards the object; and the ſame for the concave lens; muſt hold true for any uſe whatſoever, for which ſuch a formed lens may be required.

Eye glaſſes of ſpectacles.

It does not appear to have occurred to Dr. W. that the eye glaſſes uſed to magnify the images formed by the object glaſſes in teleſcopes are of the beſt form, when with the curves of the proportion above mentioned. In the eye pieces of the beſt achromatic teleſcopes, they are always applied, and in high powers, the image frequently ſubtends an angle from the centre of the eye glaſs of ſixty degrees or more. I have never ſeen any correct dioptrical theorem, that tended to prove that a meniſcus, ſingly, or combined, will anſwer ſo perfectly the ſame purpoſe.

Remarks on lenſes; to aſcertain the value of Dr. Wollaſton's patent ſpectacles.

The ordinary purpoſes of viſion, are very well anſwered by the common glaſſes, under an angle as large as eighty or ninety degrees, and the beſt artiſts, or draughtſmen allow, that 60° is as much as a fixed poſition of the eye ought in perſpective to embrace, to convey a juſt repreſentation on the optic nerve.

To perſons, the humours of whoſe eyes are ſo decayed as to be deficient in their original refractive power, glaſſes of ſhort foci

foci will, to them, render the extreme parts of objects ſomewhat confuſed; but in a much leſs degree than to perſons, with perfect eyes, or undecayed humours.

Remarks on lenſes; to aſcertain the value of Dr. Wollaſton's patent ſpectacles.

In teleſcopes and microſcopes, the aberration is uſually cut off by the inſertion of circular apertures or ſtops; but in ſpectacles this is not eſſentially neceſſary, nor does the want of them or the figure of the glaſſes, prove that they are conſtitutionally bad and prejudicial.

The obſervation of Dr. W. that only a portion of the glaſs a little larger than the pupil of the eye, is employed at once, is only juſt inaſmuch as it relates to the mind, being intent on a point of an object; but not ſo in regard to a general view. For the refractive power of the lens does moſt admirably collect all the infinite number of pencils of rays, or cones, into one concurrence at the pupil of the eye; where they croſs or interſect each other: yet, ſuch is the exquiſite ſubtilty of light, that no confuſion or irritation takes place. Man is thus bleſſed by aſſiſted viſion, as he is in vitality by the reſpiration of air.

Dr. W's inferring the form of a meniſcus from the ſhape of a globe, is manifeſtly erroneous; and in reſpect to ſpectacles inappropriate. A glaſs globe or ſphere without any ſenſible thickneſs, to an eye exactly placed in its centre, admits all the incident rays to paſs throughout it unrefracted. If the eye deviates from the centre, a refraction will take place, and that in proportion to the thickneſs of the ſphere. Rays of incidence paſs perfectly unrefracted through truly ground plane, or parallel glaſs, to an eye before it; and let the axis of the eye, be ever ſo much incloſed, unleſs the glaſs be very thick, the object will ſtill appear perfect, and no refraction of the incident rays will be obſerved. It is obvious, therefore, the nearer a lens approaches to the figure of a plane, the more perfect it muſt be.

The figure of a meniſcus, which Dr. W. wiſhes to adopt, is as different from a ſphere, as a plane. Its figure is compoſed of two portions of ſpheres, of different radii. When with a poſitive focus it is mathematically demonſtrable, that it has entirely the properties of a convex lens, and with a negative focus the properties of a concave one. When the radius of the exterior curve, is leſs than that of the interior it is a convex ſort of lens, and magnifies; but, when the radius of the interior curve is leſs than that of the other, it is

Remarks on lenſes; to aſcertain the valve of Dr. Wollaſton's patent ſpectacles.

a concave lens, and diminiſhes. It has alſo been demonſtrated, that the nearer the form of a meniſcus approaches to that of a plano convex or concave, the more perfect it will be, and produce leſs aberration.

I ſhall diſpenſe here with the proofs by algebraical and analytical formulæ, as any qualified reader will find them in the optical works of *Huygens, Molineux, Euler, D'Alembert, Smith, Emerſon and Martin*; and many others.

The rays of light iſſuing from a near object to a ſpectacle glaſs before the eye, are in diverging pencils or cones, and the meniſcus form of glaſs of any certain poſitive focus, will refract them towards a ſtate of parallelism into the eye, neceſſary to produce diſtinct viſion, in decayed ſight, preciſely in the ſame manner as a double convex or plano convex glaſs of the ſame focus would do. A meniſcus with a negative focus, acts no ways different from the double or plano concave glaſſes; the rays of light being divergent ſomewhat to counteract the effects of too great a convexity in the humours of the eye of a ſhort ſighted perſon. Perhaps, it is hardly neceſſary to obſerve, that imperfect viſion in the optical ſenſe, conſiſts in the long ſighted eye, in the rays of light not being ſufficiently converged by its humours, to meet on the retina of the eye, but falling beyond it; and in a ſhort ſighted eye by the rays converging too much, ſo as to meet before they reach the retina.

Varying the geometrical figure of a lens, does not conſtitute any new optical principle, for any of the common ſpecies of lenſes, may be cut into the form of a ſquare, a triangle, an oval, &c. all figuratively various, but conſiſting only of one optical principle.

The uſe of the meniſcus has been abandoned by Opticians, by its containing in compariſon with other lenſes, the greateſt ſpherical ſurface, and conſequently producing the greateſt aberration. Reducing the curvatures of the meniſcus elongates the focus, in the ſame manner as in other lenſes, and therefore reduces the aberration. Hence in ſpectacle-glaſſes that are not of ſhort foci, no preceptible difference will be found to perſons unacquainted with optical experiments.

There are various practical methods that will point out to perſons the aberration of lenſes here ſpoken of, and that the

meniſcus

meniſcus cauſes the greateſt of any of the other forms of lenſes; but, the following, I would recommend as the moſt eaſy and illuſtrative.

Remarks on lenſes; to aſcertain the valve of Dr. Wollaſton's patent ſpectacles.

Take a meniſcus lens about the ſize of a ſpectacle-glaſs, and with four inches poſitive focus, and take alſo a plano convex or a double convex, of the ſame diameter and focus, in a room with one lighted candle, at a diſtance by night, hold the convex glaſs near to the white wall or wainſcot ſide of the room, between it and the candle; move the lens backwards and forwards till a clear image of the candle is formed, which will be a diſtinct inverted image of it: do the ſame with a meniſcus, and there will be this difference obſerved by the meniſcus, that incircling the vivid image of the flame, there will be a faint white light, which is the circle of the aberration; and, evidently ſhews, that it is the worſt form of the two for a ſpectacle-glaſs, or any other purpoſe.

Two convex double glaſſes placed together in one cell, contain leſs aberration than one glaſs of the ſame diameter and focus; and two plano convex-glaſſes with their convex ſides placed together in one cell, give ſtill leſs aberration. It is loſs of light only that can be objected to. They are too weighty to be adopted in ſpectacles, but in eye pieces of large teleſcopes for viewing celeſtial objects, they have been uſed with great advantage. To engravers, miniature painters, and other artiſts, they are moſt uſeful, as by ſhort foci and large apertures, they give them the moſt diſtinct view of a large ſurface placed before them.

For the ſatisfaction of an intelligent perſon, who may be diſpoſed to have an ocular proof of the properties of glaſſes as herein advanced, I have conſtructed a frame, containing a double convex, plano convex, a meniſcus, two plano convexes, with their convex ſides to each other, all of the ſame diameter, and the foci about four inches, and by which may be ſeen that the greateſt peripheretical indiſtinctneſs is with the meniſcus glaſs. This apparatus will be ſhewn by application at our manufactory in Holborn.

The meniſcus as a figure for a ſpectacle glaſs I conſider very objectionable. To afford a large field of view, its diameter muſt be conſiderable; which for a ſhort focus will increaſe thickneſs, protuberance outwards, and weight; and, in concave glaſſes occaſion the frames to be made thicker; the glaſſes will

will be more liable to be ſcratched and broken than thoſe of the common form, and when the frames are metallic, more likely to increaſe than diminiſh that indelible mark made on the noſe by the weight of the frame, ſo frequently complained of by perſons who wear ſpectacles almoſt conſtantly. A great deal of ſuperfluous light alſo paſſing through the glaſſes, muſt be evidently prejudicial; and, it appears to me that the concave figure of the inner ſide of the meniſcus will act as a powerful reflector to condenſe the rays of light and heat upon the eyes, and ultimately prove thereby of ſerious injury.

I have in my poſſeſſion a meniſcus ſpectacle glaſs, taken from a ſpectacle frame, which I can prove to have been made many years ago; and, finally, as this form is neither new in principle, or in practice, I am at a loſs to know upon what ſort of diſcovery his Majeſty's letters patent have been ſolicited.

I am,

Sir,

Your's, &c.

W. JONES.

VIII.

An Account of an Occultation of b Nebulae Sagittarii, by the Planet Mars, on the 17th of April 1796, obſerved by Sir HENRY ENGLEFIELD, *Bart. F. R. S. In a Letter addreſſed to the Rev.* NEVIL MASKELYNE, *D. D. F. R. S. and Aſtronomer Royal.* *

DEAR SIR,

Occultation of a fixed ſtar by the planet Mars.

I BEG leave to communicate to you an obſervation, which I had the good fortune to make, of an occultation of a fixed ſtar by the planet Mars; and which, as far as I can learn, was ſeen by nobody in this country but myſelf and Lady Buck, at whoſe houſe at Peterſham I made the obſervation, and to whom I ſhowed the planet when nearly approaching to conjunction with the ſtar.

Theſe obſervations are very rare.

Obſervations of this kind may be ranked among the rareſt phenomena in aſtronomy. Mr. De La Lande records but

* From the Journals of the Royal Inſtitution, No. 16, p. 38.

four

four ſince the invention of teleſcopes. Of theſe, one only is by Mars, and that ſo long ago as about the year 1630, by Gaſſendi. The curious and very important conjunction of Mars with pſi aquarii, in October 1672, whence Caſſini had hoped to deduce, with great accuracy, the parallax of Mars and the Sun, was miſſed by bad weather at the moment of the occultation. Since that time, I do not know that any obſervation of this kind has been made.

Account of the obſervation.

The night of April 17, 1796, being very clear, I was led by mere accident to direct my teleſcope to Mars, about two o'clock in the morning; and I ſaw with equal pleaſure and ſurpriſe, a ſtar, about three of his diameters to the eaſt of him, and in a line perpendicular to his horns, he being then gibbous. It was evident that an occultation would take place, and I prepared every thing for the obſervation.

Inſtruments.

The teleſcope I uſed is an achromatic of Ramſden, the aperture 2.75, with a ſingle eye-glaſs, magnifying 100 times. The clock is a very good regulator, with a wooden pendulum, and to be depended on to leſs than 2 ſeconds in the 24 hours. The obſervations for the time were made with an excellent ſix inch Hadley's ſextant of Ramſden's, and an artificial horizon of mercury.

Place of the obſ.

The poſition of Peterſham, as deduced by a ſurvey made by me ſome years ſince, from the Royal Obſervatory at Kew, which was one of the points ſettled by General Roy in his great ſurvey, is $1' 12''$ in time weſt of the Obſervatory at Greenwich in longitude; and its latitude by the ſame ſurvey $51^\circ 26' 34''$.

Collateral circumſtances.

To return to the obſervation. The ſky, during the whole time, was perfectly free from clouds, but was remarkably full of vapours, which at times produced a greater undulation in the limb of Mars, than I almoſt ever ſaw. The wind was in the eaſt, and probably the columns of warm air blown from London, mixed very irregularly with the atmoſphere of Peterſham. This ſtate of the air, and the low altitude of Mars, whoſe ſouthern declination was $23^\circ\frac{1}{2}$, rendered it impoſſible to diſtinguiſh with certainty the ſtar, from the protuberances in the waving limb of Mars at the moment of immerſion, though the brilliant and ſilvery light of the ſtar contraſted ſtrongly with the dull red hue of the planet. The obſervation of the immerſion of the ſtar is therefore uncertain, to near a minute

of

of time. At the emerſion the ſtate of the air was rather more favourable than at the immerſion; and as the ſtar emerged from behind the dark limb of Mars at a ſenſible diſtance from his enlightened edge, this obſervation is much more certain than that of the immerſion; and is probably true, to five, or ten ſeconds at moſt.

Effect of the atmoſphere of Mars, probably.

The ſtar at its firſt emerſion, certainly appeared much fainter than it did ſoon afterwards; but I cannot pretend to decide, that this apparent diminution of its light was owing to any other cauſe than its great proximity to the ſuperior light of the planet. Yet it was at firſt ſo faint as to be very doubtfully viſible, and in leſs than a minute was very conſiderably brighter. As in this interval of time, its diſtance from the planet was not very much increaſed, it is probable that the atmoſphere of Mars might have been the cauſe, at leaſt in ſome degree, of this appearance.

Occultation of a fixed ſtar by the planet Mars.

Occultation of b ♑ by the Planet Mars, April 17, 1796, *at Peterſham.*

Immerſion of b ♑ per clock . . . . . 15^h 4′ 48″
Uncertain to nearly one minute.
Emerſion of b ♑ per clock . . . . . 15^h 24′ 28″
Certain to five, or ten ſeconds at moſt.

The ſtar paſſed to the north of the centre of Mars. The immerſion was about one third, or at moſt two fifths of his ſemidiameter north of the centre. The emerſion uot quite half the ſemidiameter north of the centre, by eſtimation.

The clock, at the time of the occultation,
was faſt on mean time . . . . . 0^h 4′ 21½″
The immerſion was therefore at . . . 15^h 0′ 26½″
The emerſion at . . . . . 15^h 20′ 6½″
Mean time at Peterſham.

And, Greenwich being 1′ 12″ in time eaſt of Peterſham, the obſervations reduced to the meridian of Greenwich are,

	Mean Time.	Apparent Time.
Immerſion . . .	15^h 1′ 31½″	15^h 2′ 28½″
Emerſion . . .	15^h 21′ 18½″	15^h 22′ 8½″

Although this obſervation is ſubject, from the circumſtances ſtated before, to an uncertainty of half a minute in time, as to the middle of the occultation; yet, as the geocentric motion

Occultation of a fixed ſtar by the planet Mars.

tion of Mars, was at this time not quite two ſeconds of a degree in three minutes, his place (that of the ſtar being accurately known), may certainly be determined by this obſervation within a ſingle ſecond; with a view, therefore, of aſcertaining the accuracy of the tables of Mars, given in the laſt edition of De la Lande's Aſtronomy, the longitude and latitude of Mars were computed with care as follow:

1796, April 17^d 15^h $12'$ $18''$ apparent time at Greenwich.

	Geoc. Long.	Geoc. Lat. Auſt.
Mars	8^s 27° $7'$ $37''$	0° $20'$ $41''$ $40'''$

uncorrected, for aberration or parallax. And April, 18^d 15^h $12'$ $18''$ his place was 8^s 27° $23'$ $2''$ 0° $22'$ $58''$ $18'''$.

His daily motion in longitude, was therefore 0° $15'$ $25''$, and in latitude 0° $2'$ $16''$ $38'''$ increaſing.

His aberration in longitude at the time of obſervation, was $-3''.97$, and his horizontal parallax, that of the Sun being taken at $8''.72$, was $11''.712$.

The parallax in longitude and latitude, computed by the tables of the Nonageſimal for Greenwich, given in the *Connaiſſance des Tems* for 1775 is, Par. Long. $+1''.772$, and Par. Lat. $+11''.2$.

The apparent place of Mars at the time of the occultation was, therefore, by theſe tables,

Geoc. Long.	Geoc. Lat. Auſt.
8^s 27° $7'$ $34''.8$	0° $20'$ $52''.9$.

But the place of the ſtar, as ſettled at Greenwich, was at the time of occultation,

Long.	Lat. Auſt.
8^s 27° $6'$ $19''$	0° $20'$ $41''$

And as the centre of the planet paſſed about two fifths of his ſemidiameter ſouth of the ſtar, and the apparent diameter of Mars at that time was very nearly fourteen ſeconds and an half; the difference of latitude between the ſtar and Mars, may be eſtimated at three ſeconds, without poſſible error of above half a ſecond.

The apparent place then of Mars, as found by obſervation, was,

Geoc. Long.	Geoc. Lat. Auſt.
8^s 27° $6'$ $19''$	0° $20'$ $41''$

and the error of the tables is in longitude $+1'$ $15''.8$, and in latitude $+12''$.

Had

Occultation of a fixed ſtar by the planet Mars.

Had this obſervation been made under favourable circumſtances, the diameter of Mars might probably have been deduced from it with a great degree of accuracy; although ſuch determination would always have been ſubject to uncertainty, from the effects of the refraction of his atmoſphere, which it is impoſſible to eſtimate, but which ſeems, from ſome circumſtances, to be very conſiderable.

In the preſent obſervation, two other cauſes of uncertainty exiſt. The firſt is, the doubt of the duration of the occultation; the ſecond, the poſſibility of miſtake in the eſtimation of the diſtance of the ſtar from the centre of Mars.

The firſt of theſe cauſes cannot induce an error of more than two thirds of a ſecond in the eſtimate of his diameter. Probably the error is not more than half that quantity.

The probable error ariſing from the ſecond cauſe, may amount to about the ſame quantity as the former. Of this I formed as near a judgment as I could, by drawing Figure 2, *Plate XII.* and placing the path of the ſtar at its utmoſt limits of diſtance from the centre by my eye. If the effects of both theſe cauſes lie the ſame way, an error of a ſecond and an half in the determination of the diameter of Mars, may poſſibly ariſe; but it is highly probable, that the error is very much leſs than that quantity, putting the effects of refraction out of the queſtion.

Having, therefore, to the beſt of my power, compared all the circumſtances of the obſervation, and the effects of the above cauſes; I conſider the equatorial diameter of Mars, as reſulting from it, to be at that time 14″.5: and as the diſtance of Mars from the Earth was then 0.74456, the Sun's mean diſtance being 1, the diameter of Mars at the mean diſtance of the Sun, would be 10″.8. Dr. Herſchel, in his laſt paper on Mars, makes it 9″.8.

As all the cauſes of error in my obſervation, tend rather to diminiſh than increaſe the ſize of the planet, I am ſurpriſed that I ſhould have exceeded his eſtimate; which is, however, leſs than that of former aſtronomers.

This, Sir, is all that has occurred to me on the ſubject of this very uncommon obſervation.

You will perceive, that I have not, in my eſtimations of the diameter of Mars, or of the diſtances of the ſtar from his limb, made any mention of the ſpheroidical figure of the planet. The differences in the reſults would be perfectly inſenſible,

fible, if that element had been conſidered; and as I never have been able to perceive the difference of his diameters, I cannot but ſuſpect it to be leſs than Dr. Herſchel ſuppoſes. Occultation of a fixed ſtar by the planet Mars.

I am,

Dear Sir,

Your obliged and faithful,

May 23, 1797. H. C. ENGLEFIELD.

P. S. In the *Connaiſſance des Tems* for the year VI. page 457, and year VII. page 428, are accounts of an obſervation of this occultation, made at Aubenas, or Viviers (for it is not certain at which of theſe places) by Mr. Flaugergue, member of the National Inſtitute. It is not clear from theſe accounts that he actually ſaw the emerſion: at $15^h\ 58'\ 25''$, apparent time, he ſaw the ſtar, diſtant from the limb of the planet only by an interval equal to the ſuppoſed breadth of the dark part of the diſc of Mars, and it was extremely faint, but perfectly white. Its poſition was about 20° to the weſt of the vertical, and above the horizontal diameter. Mr. Flaugergue ſuppoſes that the ſtar paſſed within a ſecond of the northern limb of Mars, and muſt have been hidden ten minutes and an half.

As the place of obſervation was in a lower latitude than London, the obſerver there muſt have ſeen the ſtar paſs nearer the centre of Mars than I did, the effect of parallax being leſs, and the occultation muſt in conſequence have laſted longer: but my obſervation proves, beyond a doubt, that the ſtar paſſed much nearer the centre than Mr. Flaugergue ſuppoſes, as I loſt ſight of the ſtar at leaſt nineteen minutes and an half.

IX.

Letter from Mr. EZEKIEL WALKER, *containing a conſiderable Improvement in Time-Pieces by Mr.* BARRAUD, *with Remarks.*

To Mr. NICHOLSON,

SIR,

THE method of determining the longitude by time-keepers, poſſeſſes ſome advantages ſuperior to any other that has yet been propoſed. In the firſt place, this method is attended with The determination of longitude by a time-keeper is more eaſy and can be oftener

made than by other methods.

with very little trouble; and, ſecondly, the longitude can be found oftener by a time-keeper than by any other means. I have not met with a more ſtriking proof of this poſition, than in the voyage of La Perouſe round the world in the ſhip La Bouſſole. Between the 1ſt of Auguſt 1785, and the 8th of September 1787, this celebrated navigator took only 72 lunar obſervations; but he took 393 obſervations on the longitude by his time-keeper No. 19. This is an unequivocal proof of the benefits that navigation derives from this mode of finding the longitude.

Remarkable caſe in proof.

But time-keepers are very expenſive, and liable to irregularity.

It is to be regretted, however, that time-keepers are expenſive and liable to ſtop, or go irregularly. Were it not for theſe inconveniences, no other method of finding the longitude need be ſought after. But a diſcovery was made by Mr. Barraud, about the cloſe of the laſt century, on the effects of oil on time-keepers, which not only reduces them in price, but contributes to their performing with greater preciſion.

Diſcovery of Mr. Barraud, by which time-keepers are rendered more perfect, and cheaper.

Mr. Barraud had frequently communicated to me his improvements in chronometry, but this diſcovery on the effects of oil, appeared a matter of ſo much importance, that I wrote to him, requeſting his leave to publiſh it, and the following extract is taken from his obliging anſwer.

To Mr. WALKER.

DEAR SIR, *London, Jan.* 28, 1804.

Letter from Mr. Barraud.

I AM much pleaſed to find that it is your intention to favour the public with your obſervations on chronometry, and ſhall derive ſatisfaction in contributing my mite to ſo deſirable an end: you are therefore welcome to uſe ſuch information as my experience of facts enables me to furniſh.

Jewelling the holes of time-keepers is injurious;

the oil in ſuch holes being ſooner vitiated than in braſs holes.

The ſtate of my regulator I have already deſcribed [in a former letter], from which I infer, that ſo far from jewelled holes being advantageous in clock-work, they are abſolutely injurious. That they are equally ſo in chronometers, I have had abundant experience, having found, almoſt without exception, in chronometers coming off a long voyage, the oil in a much worſe condition in the jewelled holes (particularly in thoſe where the friction was conſiderable) than in the braſs ones. I have therefore been induced, in every inſtance, to reject jewelled holes, and introduce thoſe of braſs, and the

alteration

alteration has been conſtantly favourable to the performance of the time-keeper. I ſhould be happy in having a communication with you *viva voce* on this theme, &c. &c.

I am, &c.

P. P. BARRAUD.

That Mr. Barraud has not made this alteration in his chronometers, in a haſty manner, will appear from the following extract taken from another of his letters.

To Mr. WALKER.

DEAR SIR, *London, July* 17, 1800.

SINCE we parted, I have found additional reaſons to believe that jewelled holes (where friction is great) are injurious. A box time-keeper which I have recently taken to pieces, on its return from a long voyage, had the oil in the braſs holes in a much purer ſtate than in the jewelled ones. In the former it ſtill remained in a ſtate favourable for action, but in the latter, the pivots were ſo fixed by the tenacity of the oil, as to require force to extricate them; the ſteel was alſo deeply ſtained, and had parted with all its luſtre, &c. &c.

Inſtance of a time-keeper, in which the oil was much more vitiated in the jewelled than in the braſs holes:

I am, &c.

P. P. BARRAUD.

From theſe and many other obſervations made by Mr. Barraud upon the effects of oil on time-keepers, it appears that ſmall particles of ſteel are worn off by friction in the jewelled holes, and mixing with the oil, form a glutinous ſubſtance that cauſes the time-keeper to go irregularly.

Moſt probably by abraſion of the ſteel pivots.

On the Action of Cold on Oils.

IT is a known property of ſome oils, that they freeze much ſooner than water. The oil of olives freezes at $42\frac{1}{2}^{\circ}$ on Fahrenheit's ſcale, conſequently the pureſt oil of this kind will loſe its fluidity ſooner than that which contains ſome aqueous particles. Hence we may conclude, that watch-makers ought to make choice of that oil which freezes with the leaſt degree of cold; and as cold has no power to decompoſe olive oil, it need not be rejected on account of its having aſſumed the concrete form.

Olive oil freezes at $42\frac{1}{2}$ degrees.

In

In conſequence of this oil freezing much ſooner than water, the following queries ſeem to claim our attention:

Hence it may perhaps be neceſſary to adjuſt and keep time-keepers at temperatures between 43° and 100°.

Query 1. Will a time-keeper go at the ſame rate when the oil is frozen, that it did when the oil was in a fluid ſtate?

If this query be anſwered in the negative, *Query* 2. Would it not then be improper to adjuſt the compenſation balance in froſty weather?

Query 3. Would it not be better to adjuſt the compenſation for the effects of heat and cold at 43° for the greateſt degree of cold, and at 96 or 100° for the other extreme?

A chronometer adjuſted in this manner, ſhould never be expoſed to a greater degree of cold than 43. This may be eaſily done with pocket chronometers, but to keep the oil from freezing in box time-keepers, in cold climates, more care may be requiſite.

Oil has other properties which ought to be carefully examined before it be applied to time-keepers, but this is an inquiry which muſt be left until ſome future opportunity.

I am, Sir,

With much reſpect,

Your humble ſervant,

E. WALKER.

X.

Letter from C. WILKINSON, *Eſq. containing Facts upon which Deductions are made to ſhew the Law of Galvaniſm in burning the Metals, according to the Diſpoſition of equal Surfaces of charged Metallic Plate.*

To Mr. NICHOLSON,

SIR,

IF the following obſervations ſhould be deemed worthy of inſertion in your valuable Journal, I ſhall take the liberty of troubling you with ſome further remarks hereafter.

I am, Sir,

Yours reſpectfully,

Soho Square. C. WILKINSON.

Introduction.

When the French philoſophers had aſcertained that a ſeries of galvanic plates produced effects on animal ſubſtances in proportion to the number of plates employed, without any regard to the ſurface of each plate, they concluded from their experiments, that the effects of a galvanic battery on metallic ſubſtances are in proportion to the ſurfaces of the plates employed. I have lately been engaged in experiments with the moſt extenſive galvanic apparatus hitherto conſtructed, from which ſome circumſtances have occurred, in ſome reſpects militating with the deductions of the French philoſophers.

One hundred galvanic plates of four inches ſquare burned half an inch of wire.

A galvanic trough containing one hundred ſquare plates of four inches in the ſide, each plate formed of a plate of zinc and copper ſoldered together; when charged with a ſolution of nitrous acid and water in the proportion of about 25 parts water to one of acid, exhibited a power capable of igniting half an inch of ſteel wire of about one ſeventieth of an inch in diameter.

Other troughs added endwiſe burned more wire in proportion to the number of plates.

When two ſuch troughs were combined endwiſe, the power was doubled, and when four were thus arranged, the quantity of wire ignited was quadrupled: hence I aſcertained in a very extenſive arrangement, that the power is invariably in proportion to the number of plates employed.

A trough of larger plates had much more power,

A galvanic trough conſiſting of fifty ſquare plates of eight inches each in the ſide was charged with a ſimilar prepared ſolution, and this arrangement I found capable of igniting ſixteen inches of the ſame wire as was employed in the former experiment.

—which increaſed proportionally to the number.

When two, three, and four troughs of the ſame ſize were combined, the lengths of wire ignited proved to be in proportion to the number of plates employed; ſo that two hundred eight inch plates ignited more than five feet of wire.

Examination of the law of increaſe in the power of larger plates.

Theſe experiments prove that the powers increaſe in a greater ratio than as the ſurfaces of the plates employed. For four hundred plates of four inches, contain an equal ſurface with one hundred plates of eight inches; and the former will only ignite two inches of the ſame wire, of which the latter will ignite thirty-two inches.

By equal total ſurfaces the wire burned is as the ſquare of the elementary ſurface.

If this proportion ſhould be obſerved in experiments with plates of different ſize, it will appear that the powers of igniting, as meaſured by the length of wire, increaſe, in batteries of the ſame total ſurface, as the ſquares of the ſurfaces of the elementary plates, ſingly taken in each.

A plate

Reaſoning on the facts. Large plates throw our electricity more rapidly.

A plate of eight inches diameter expoſes a ſurface four times greater than a plate of four inches, and ſuppoſing the quantities of electricity given out, to be in proportion to the ſurface expoſed to the chemical action of the fluids, the intenſity of (or rapidity of evolution from) each plate of an eight inch trough may be eſtimated as four times greater than the intenſity of electricity from the four inch plates; but the power of ignition is ſixteen times greater.

Upon theſe remarkable circumſtances I ſhall venture to ſubmit to you a few conjectures.

The electricity is thrown out by the parts under the proceſs of oxidation,

A ſimple galvanic combination reſembles in its properties a Leyden phial, that ſide of the metallic plate which undergoes the greateſt change reſembles the poſitive ſide of the jar, and the other ſide, the negative, there being no other difference between electricity and galvaniſm, than the mode in which they are produced. Electricity which appears from the excitation of glaſs, is occaſioned by a temporary change in the capacity of the glaſs for electricity, while under the rubbing action, being momentarily increaſed; while galvaniſm is in ſome reſpects the reverſe, being occaſioned by the diminiſhed capacity of good conducting bodies, when expoſed to certain chemical changes. Thus metals which are excellent conductors, when oxydated become non-conductors. While they undergo this change a portion of the combined electricity is evolved, and conſequently in the part thus changing poſitive ſigns will be evinced.

—and thus by electrophoric action produces a negative ſtate on the unoxided ſurface.

The electricity exiſting in an unchanged metal being perfectly quieſcent, is ſubject, when diſturbed, to thoſe laws to which all material ſubſtances are amenable; namely of moving in the direction where it meets with the leaſt reſiſtance. As the direction of the electricity is from the ſubſtance of the metal towards the ſurface undergoing the chemical change, it muſt follow that the other ſide of the metallic plate which is defended from the action of the fluid will evince negative ſigns, while the ſide acted upon poſitive, nearly upon the ſame principles as thoſe of the electraphams.

The diſcharge is by a ſucceſſion of correſpondent pairs of plates,

In effecting a diſcharge of the galvanic battery, the circuit muſt be completed between the zinc and copper ends of the battery. The two plates neareſt the points of contact are firſt concluded; thus the two next, and ſo on in ſucceſſive parts, each plate at equal diſtances from the points of contact; and

the laſt pair which is unloaded, are the two plates in the centre of the trough. In an arrangement of only 50 or 60 plates, the rapidity of each ſucceſſive diſcharge appears to be ſo great as not to admit of the intervals being diſtinguiſhed by our ſenſe of feeling; but when the number is very conſiderable the ſenſation produced is jarring and tremulous, ſuch as we may conceive would ariſe from the action of a conſtant current of a ſubtile fluid.

—conſequently the whole diſcharge of equal ſurfaces is more rapidly made the fewer the plates,

When we expoſe metallic ſubſtances to the galvanic action, the effects produced are greater from a large ſurface diffuſed over a ſmall number of plates, than when ſpread over a greater number of plates; if we ſuppoſe 800 particles of electricity given out from a plate of eight inches in diameter, there would only be 200 particles from a plate of four inches (ſuppoſing the quantity in the ratio of the ſurfaces expoſed) if two plates be unloaded at once, we ſhall have from one arrangement 1600 particles of electricity determined in one maſs, and from the other only 400; from the former we have a rapid communication of a quantity of electricity of four times the intenſity of the latter. And as metallic ſubſtances nearly tranſmit the whole, the effects produced are found to be as the ſquares of the intenſities.

—and the quantity of electricity actually paſſing in the wire at any inſtant of time is greater.

I am now conſtructing a battery of fifty plates, each plate of which will be two feet in diameter. If the above circle ſhould hold good, the extent of wire that will be ignited will be very conſiderable. A battery of 50 plates, each plate eight inches in diameter, ignites 16 inches of ſteel wire of one ſeventieth of an inch in diameter. And as the plates I am now preparing will expoſe a ſurface nine times greater, I ſhall expect that $16 \times 9 \times 9 = 1296$ inches, or 108 feet of wire will be ignited at every contact.

A battery now in hand expected to explode above 100 feet of wire.

XI.

Account of the Changes that have happened, during the laſt Twenty-five Years, in the relative Situation of Double-ſtars; with an Inveſtigation of the Cauſe to which they are owing. By WILLIAM HERSCHEL, *LL. D. F. R S. From the Philoſophical Tranſactions for* 1803.

Read June 9, 1803.

General obſervations on the theory of the heavenly bodies.

IN the Remarks on the Conſtruction of the Heavens, contained in my laſt Paper on this ſubject,* I have divided the various objects which aſtronomy has hitherto brought to our view, into twelve claſſes. The firſt comprehends inſulated ſtars.

As the ſolar ſyſtem preſents us with all the particulars that may be known, reſpecting the arrangement of the various ſubordinate celeſtial bodies that are under the influence of ſtars which I have called inſulated, ſuch as planets and ſatellites, aſteroids and comets, I ſhall here ſay but little on that ſubject. It will, however, not be amiſs to remark, that the late addition of two new celeſtial bodies, has undoubtedly enlarged our knowledge of the conſtruction of the ſyſtem of inſulated ſtars. Whatever may be the nature of theſe two new bodies, we know that they move in regular elliptical orbits round the ſun. It is not in the leaſt material whether we call them aſteroids, as I have propoſed; or planetoids, as an eminent aſtronomer, in a letter to me, ſuggeſted; or whether we admit them at once into the claſs of our old ſeven large planets. In the latter caſe, however, we muſt recollect, that if we would ſpeak with preciſion, they ſhould be called very ſmall, and exzodical; for, the great inclination of the orbit of one of them to the ecliptic, amounting to 35 degrees, is certainly remarkable. That of the other is alſo conſiderable; its latitude, the laſt time I ſaw it, being more than 15 degrees north. Theſe circumſtances, added to their ſmallneſs, ſhow that there exiſts a greater variety of arrangement and ſize among the bodies which our ſun holds in ſubordination, than we had formerly been acquainted with, and extend our knowledge of the conſtruction of the ſolar, or inſulated ſidereal ſyſtem. It will not be required that I ſhould add any

* See Phil. Tranſ. for 1802, p. 477, and our Journal, V. 72,

thing

thing farther on the ſubject of this firſt article of my claſſification; I may therefore immediately go to the ſecond, which treats of binary ſidereal ſyſtems, or real double ſtars.

General obſervations on the theory of the heavenly bodies.

We have already ſhewn the poſſibility that two ſtars, whatſoever be their relative magnitudes, may revolve, either in circles or ellipſes, round their common centre of gravity; and that, among the multitude of the ſtars of the heavens, there ſhould be many ſufficiently near each other to occaſion this mutual revolution, muſt alſo appear highly probable. But neither of theſe conſiderations can be admitted in proof of the actual exiſtence of ſuch binary combinations. I ſhall therefore now proceed to give an account of a ſeries of obſervations on double ſtars, comprehending a period of about 25 years, which, if I am not miſtaken, will go to prove, that many of them are not merely double in appearance, but muſt be allowed to be real binary combinations of two ſtars, intimately held together by the bond of mutual attraction.

It will be neceſſary to enter into a certain theory, by which theſe obſervations ought to be examined, that we may find to what cauſe we ſhould attribute ſuch changes in the poſition, or diſtance, of double ſtars, as will be reported; and, in order to make the required principles very clear, I ſhall give them in a few ſhort and numbered ſentences, that they may be referred to hereafter.

Theory of the motions of the ſtars.

In *Plate.* X. *Fig* 1. let us call the place of the ſun, which may alſo be taken for that of the obſerver, O. In the centre of an orbit or plane N F S P is α Geminorum; and, if any other ſtar is to be examined, we have only to exchange the letter α for that by which ſuch double ſtar is known. This letter is always underſtood to repreſent the largeſt of the two ſtars which make up the double ſtar; and a general expreſſion for its ſmaller companion will be x. N, F, S, P, repreſent the poſitions of the different parts of the heavens, with reſpect to α, north, following, ſouth, and preceding; and the ſmall letters n, f, s, p, ſtand for the ſame directions with reſpect to O. $x\,\alpha\,P$, is the angle of poſition of the two ſtars x and α, with the parallel F P.

As the motion of an obſerver affects the relative ſituation of objects, we have three bodies to conſider, in our inveſtigation of the cauſe of the changes which will be pointed out; the ſun, the large ſtar, and the ſmall ſtar, or, as we have ſhortly called them,

Theory of the motions of the ſtars.

them, O, α, x. This admits of three caſes: a motion of one of the three bodies; another, of two; and a third, of all the three bodies together. We ſhall now point out the conſequences that will ariſe in each of the caſes.

Single Motions.

No. 1. Motion of x. When α and O are at reſt, the motion of x may be aſſumed, ſo as perfectly to explain any change of the diſtance of the two ſtars, and of their angle of poſition.

No. 2. Motion of α. When x and O are at reſt, and α has a motion, either towards P, N, F, or S, then the effect of it, whatever may be the angle P α O, will be had by entering the following Table, with the direction of the given motion.

Motion.	Diſtance.	Angle.	Quadrants.
αP	$-$ $+$	$+$ $-$	1ſt and 4th 2 —— 3
αF	$+$ $-$	$-$ $+$	1 —— 4 2 —— 3
αN	$-$ $+$	$-$ $+$	1 —— 2 3 —— 4
αS	$+$ $-$	$+$ $-$	1 —— 2 3 —— 4

No. 3. Motion of O. 1ſt caſe. When α and x are at reſt, and the angle P α O is 90 degrees, a proper motion of O, towards either p, f, n, or s, which will be extremely ſmall when compared with the diſtance of O from α, can have no effect on the apparent diſtance, or angle of poſition, of the two ſtars; and therefore no other motion, compoſed of the directions we have mentioned, will induce a change in the comparative ſituation of α and x.

2d caſe. When the plane P N F S is oblique to the ray α O, and the angle P α O more than 90 degrees, the effect of the motion of O will be had by the following Table.

Motion

Theory of the motions of the ſtars.

Motion.	Diſtance.	Angle.	Quadrants.
O *p*	+	—	1ſt and 2d 3 —— 4
O *f*	—	+	1 —— 2 3 —— 4
O *n*	+ —	+ —	1 —— 3 2 —— 4
O *s*	— +	— +	1 —— 3 2 —— 4

3d caſe. When the angle P ∝ O is leſs than 90 degrees, the following Table muſt be uſed.

Motion.	Diſtance.	Angle.	Quadrants.
O *p*	—	+	1ſt and 2d 3 —— 4
O *f*	+	—	1 —— 2 3 —— 4
O *n*	— +	— +	1 —— 3 2 —— 4
O *s*	+ —	+ —	1 —— 3 2 —— 4

Double Motions.

No. 4. If we admit different motions in two of our three bodies, and if the ratio of the velocities, the directions of the motions, and the ratio of the diſtances of the bodies be given quantities, a ſuppoſition in which we admit their concurrence, may explain the phenomena of a double ſtar, but can never be probable

Motions of the three Bodies.

No. 5. If we admit different motions in every one of the three bodies, O ∝, *x*, and if the velocities and directions of the motions,

Theory of the motions of the ſtars.

motions, as well as the relative diſtances of the three bodies are determined, an hypotheſis which admits the exiſtence of ſuch motions and ſituations, may reſolve the phenomena of a double ſtar, but cannot have any pretenſion to probability.

The compaſs of this Paper will not allow me to give the obſervations of my double ſtars at full length; I ſhall therefore, in the examination of every one of them, only ſtate thoſe particulars which will be required for the purpoſe of inveſtigating the cauſe of the changes that have taken place, either in the diſtance, or angle of poſition, of the two ſtars of which the double ſtar is compoſed.

As the arguments in the caſe of moſt of theſe ſtars will be nearly the ſame, it may be expected, that the firſt two or three which are to be examined will take up a conſiderable ſpace, and the number of double ſtars, in which I have already aſcertained a change, amounting to more than fifty, it will not be poſſible to give them all in one paper; I ſhall therefore confine the preſent one to a moderate length, and leave it open for a continuation at a future opportunity.

α Geminorum.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

From my earlieſt obſervations on the diſtance of the two ſtars which make up the double ſtar in the head of Caſtor, given in the firſt of my catalogues of double ſtars, we find, that about 23 years and a half ago, they were nearly two diameters of the large ſtar aſunder. Theſe obſervations have been regularly continued, from the year 1778 to the preſent time, and no alteration in the diſtance has been perceived: the ſtars are now ſtill nearly two diameters of the large one aſunder.

It will be neceſſary to enter a little into the practicability of aſcertaining diſtances by a method of eſtimation apparently ſo little capable of preciſion. From a number of obſervations and experiments I have made on the ſubject, it is certain that the apparent diameter of a ſtar, in a reflecting teleſcope, depends chiefly upon the four following circumſtances: the aperture of the mirror with reſpect to its focal length; the diſtinctneſs of the mirror; the magnifying power; and the ſtate of the atmoſphere at the time of obſervation. By a contraction of the aperture we can increaſe the apparent diameter of a ſtar,, ſo as to make it reſemble a ſmall planetary diſk. If diſtinctneſs ſhould be wanting, it is evident that the image of objects will not be ſharp

and

and well defined, and that they will conſequently appear larger than they ought. The effect of magnifying power is, to occaſion a relative increaſe of the vacancy between two ſtars that are very near each other; but the ratio of the increaſe of the diſtance is not proportional to that of the power, and ſooner or later comes to a maximum. The ſtate of the atmoſphere is perhaps the moſt material of the four conditions, as we have it not in our power to alter it. The effects of moiſture, damp air, and hazineſs, (which have been related in a paper where the cauſes that often prevent the proper action of mirrors were diſcuſſed,) ſhow the reaſon why the apparent diſtance of a double ſtar ſhould be affected by a change in the atmoſphere. The alteration in the diameter of Arcturus, extending from the firſt to the laſt of the ten images of that ſtar, in the plate accompanying the above-mentioned paper,* ſhows a ſufficient cauſe for an increaſe of the diſtance of two ſtars, by a contraction of their apparent diſks. A ſkilful obſerver, however, will ſoon know what ſtate of the air is moſt proper for eſtimations of this kind. I have occaſionally ſeen the two ſtars of Caſtor, from $1\frac{1}{2}$ to 2 and $2\frac{1}{2}$ diameters aſunder; but, in a regular ſettled temperature and clear air, their diſtance was always the ſame. The other three cauſes which affect theſe eſtimations, are at our own diſpoſal; an inſtance of this will be ſeen in the following trial. I took ten different mirrors of ſeven feet focal length, each having an aperture of 6,3 inches, and being charged with an eye-glaſs which gave the teleſcope a magnifying power of 460. With theſe mirrors, one after another, the ſame evening I viewed the two ſtars of our double ſtar; and the reſult was, that with every one of them, the ſtars were preciſely at an equal diſtance from each other. Theſe mirrors were all ſufficiently good to ſhow minute double ſtars well; and ſuch a trial will conſequently furniſh us with a proper criterion, by which we may aſcertain the goodneſs of our teleſcope, and the clearneſs of the atmoſphere required for theſe obſervations. To thoſe who have not been long in the habit of obſerving double ſtars, it will be neceſſary to mention, that, when firſt ſeen, they will appear nearer together than after a certain time; nor is it ſo ſoon as might be expected, that we ſee them at their greateſt

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

* See Phil. Tranſ. for 1803, page 232, Plate III.

diſtance.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

diſtance. I have known it to take up two or three months, before the eye was ſufficiently acquainted with the object, to judge with the requiſite preciſion.

Whatever may be the difficulties or uncertainties, attending the method of determining the diſtance of two cloſe ſtars by an eſtimation of the apparent diameter, it muſt however be confeſſed, that we have no other way of obtaining the ſame end with ſo much preciſion. Our preſent inſtance of α Geminorum, will ſhow the degree of accuracy of which ſuch eſtimations are capable, and at the ſame time prove, that the purpoſe for which I ſhall uſe the eſtimated interval between the two ſtars will be ſufficiently anſwered. By an obſervation of the 10th of May, 1781, we have the diameter of the largeſt of the two ſtars to that of the ſmalleſt as 6 to 5; and according to ſeveral meaſures I have taken with the micrometer, we may admit their diſtance, diameters included, to be five ſeconds. Then, as the vacancy between the two ſtars is nearly, but not quite two diameters of the large one, I ſhall value it at $1\frac{7}{8}$. From this we calculate, that the diameter of the large ſtar, under the circumſtances of our eſtimation, is nearly $1'',35$: ſo that an error of one quarter of ſuch a diameter, which is the moſt we can admit, will not exceed $0'',34$. Nor is it of much conſequence, if the meaſure of $5''$ ſhould not be extremely correct; as a ſmall miſtake in that quantity will not materially affect the error of eſtimation by the diameter, which, from what has been ſaid, if the meaſure was faulty to a ſecond, would not amount to more than one-fifteenth part of it.

Having thus aſcertained that no perceptible change in the diſtance of the ſtars has taken place, we are now to examine the angle of poſition. In the year 1779, it was $32^\circ\ 47'$ north preceding; and, by a mean of the three laſt meaſures I have taken, it is now only $10^\circ\ 53'$. In the ſpace of about 23 years and a half, therefore, the angle of poſition has manifeſtly undergone a diminution, of no leſs than $21^\circ\ 54'$; and, that this change has been brought on by a regular and gradual decreaſe of the angle, will be ſeen when the reſt of the meaſures come to be examined.

The accuracy of the micrometer which has been uſed, when the angles of poſition were taken, being of the utmoſt importance, it becomes neceſſary to aſcertain how far it will be ſafe to rely on the reſult of the meaſures. It might be eaſily

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shown that, in the day time, a given angle, delineated on a card, and stuck up at a convenient distance, may be full as accurately measured by a telescope furnished with this micrometer, as it can be done by any known method, when the card is laid on a table before us; but this would not answer my purpose. For, objects in motion, like the stars, especially when at a distance from the pole, cannot be measured with such steadiness as those which are near us, and at rest. The method of illuminating the wires, and other circumstances, will likewise affect the accuracy of the angles that are measured, especially when the distance of the stars is very small. I shall therefore have recourse to astronomical observations, in order to see what the micrometer has actually done.

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

January 22, 1802. The position of A Orionis was taken. 1st measure, 52° 38′ south preceding; 2d measure, 54° 14′. Mean of the two measures, 53° 26′. Deviation of the measures from the mean, 48′.

March 4, 1802. 11 Monocerotis. 1st measure, 28° 18′ south following; 2d measure, 26° 49′. Mean of the two, 27° 34′. Deviation from the mean, 45′.

February 9, 1803. α Geminorum. 1st measure, 6° 11′ north preceding; 2d measure, 4° 48′. Mean of the two, 5° 29′. Deviation from the mean, 41′.

September 6, 1802. η Coronæ. 1st measure, 89° 42′ north following; 2d measure, 89° 38′. Mean of the two, 89° 40′. Deviation from the mean, 2′.

When these observations are considered, we shall not err much if we admit that, in favourable circumstances, and with proper care, the micrometer, by a mean of two measures, will give the position of a double star true to nearly one degree; but, as the opportunities of taking very accurate measures are scarce, it will be necessary to have recourse to some more discordant observations.

February 18, 1803. β Orionis. 1st measure, 72° 58′ south preceding; 2d measure, 67° 24′. Mean of the two, 70° 11′. Deviation from the mean, 2° 47′.

But a memorandum to the observation says, that the evening was not favourable. We may therefore admit, that in the worst circumstances which can be judged proper for measuring at all, an error in the angle of position by two measures will not amount to three degrees.

Obſervations and inferences reſpecting the changes of relative ſituation ſtars extremely near each other.

It will be remarked, when we come to compare ſingle meaſures which have been taken on different nights, that they are ſomewhat more diſcordant; but I have not ventured to reject them on that account, except in caſes where it was pretty evident that ſome miſtake in reading off, or other accident to which all aſtronomical obſervations are liable, was to be apprehended. Nor can ſuch diſagreements materially affect the concluſions I have drawn, when it appears that the deviations happen ſometimes to be on one ſide, and ſometimes on the other ſide, of the true angle of poſition. For, ſince that angle is not a thing that will change in the courſe of a few nights, the exceſs of one meaſure will ſerve to correct the defect of another; and we are not to think it extraordinary, when ſtars are ſo near together, and their motion through the field of view (in conſequence of the high magnifying power we are obliged to uſe) ſo quick, that we ſhould now and then even fall ſhort of that general accuracy which may be had by a careful uſe of the micrometer.

I ſhall now enter into an examination of the cauſe of the change in the angle of poſition of the ſmall ſtar near Caſtor.

A revolving ſtar, it is evident, would explain in a moſt ſatisfactory manner, a continual change in the angle of poſition, without an alteration of the diſtance. But this, being a circumſtance of which we have no precedent, ought not to be admitted without the fulleſt evidence. It will therefore be right to examine, whether the related phenomena cannot be ſatisfactorily explained by the proper motions of the ſtars, or of the ſun.

Single Motions.

(*a*) The three bodies we have to conſider, are O, α, and x; and, ſuppoſing them to be placed as they were obſerved to be in the year 1779; the angle $x \alpha$ P, in Fig. 1, will be 32° 47′ north preceding. We are at liberty to let the angle P α O be what will beſt anſwer the purpoſe. Then, in order to examine the various hypotheſes that may be formed, according to the arrangement of the principles we have given, we ſhall begin with No. 1; and, as this admits that all phenomena may be reſolved by a proper motion of x, let us ſuppoſe this ſtar to be placed any where far beyond α, but ſo as to have been ſeen, in the year 1779, where the angle of poſition, 32° 47′ north preceding

ceding, and the obſerved diſtance, near 2 diameters of the large ſtar, required it. With a proper velocity, let it be in motion towards the place where it may now be ſeen at the ſame diſtance from Caſtor, but under an angle of poſition only 10° 53′ north preceding. It may then be admitted, that a ſmall decreaſe of the diſtance which would happen at the time when the angle of poſition was 21° 50′, could not have been perceived; ſo that the gradual change in the obſerved angle of poſition, as well as the equality of the diſtance of the two ſtars, will be ſufficiently accounted for. But the admiſſion of this hypotheſis requires, that α Geminorum and the ſolar ſyſtem ſhould be at reſt; and, by the obſervations of aſtronomers, which I ſhall ſoon have occaſion to mention, neither of theſe conditions can be conceded.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each othes.

(*b*) If, according to No. 2, we admit the motion of α, we ſhall certainly be more conſiſtent with the obſervations which aſtronomers have made on the proper motion of this ſtar *; and, as a motion of the ſolar ſyſtem, which I ſhall have occaſion to mention hereafter, has not been rigidly proved, it may, for the ſake of argument, be ſet aſide; nor has a proper motion of the ſtar *x* been any where aſcertained. The retrograde annual proper motion of Caſtor, in right aſcenſion, according to Dr. Maſkelyne, is 0″,105. This, in about 23½ years, during which time I have taken notice of the angle of poſition and diſtance of the ſmall ſtar, will amount to a change of nearly 2″,47. Then, if we enter the ſhort Table I have given in No. 2, with the motion αP, we find, that in the firſt quadrant, where the ſmall ſtar is placed, the diſtance between the two ſtars will be diminiſhed, and the angle of poſition increaſed. But ſince it appears, by my obſervations, that the diſtance of the ſtars is not leſs now than it was in 1780; and that, inſtead of an increaſe in the angle of poſition, it has actually undergone a diminution of nearly 22 degrees; it follows, that the motion of α Geminorum in right aſcenſion, will not explain the obſerved alterations in the ſituation of this double ſtar. If,

* See Tobiæ Mayeri *Opera inedita*. *De motu fixarum proprio*, page 80. Alſo Dr. Maskelyne's firſt Volume of Obſervations. Explanation and Uſe of the Tables, p. iv. Or Mr. Wollaſton's Aſtronomical Catalogue, end of the Preface. Likewiſe *Connoiſſance des Temps pour l'Année* VI. p. 203. *Sur le Mouvement particulier propre a differentes Etoiles*; par Mons. De la Lande.

according

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

according to Mr. De la Lande's account*, we ſhould alſo conſider the annual proper motion of α in declination, which is given 0″,12 towards the north, we ſhall find, by entering our Table with the motion αN, amounting to 2″82, that the diſtance of the two ſtars will be ſtill more diminiſhed; but that, on the contrary, the angle of poſition will be much leſſened; and, by combining the two motions together, the apparent diſks of the two ſtars ſhould now be a little more than one-tenth of a ſecond from each other, and the angle of poſition 35 degrees ſouth preceding. But, ſince neither of theſe effects have taken place, the hypotheſis cannot be admitted.

(c) That the ſun has a proper motion in ſpace, I have ſhown with a very high degree of evidence, in a paper which was read at the Royal Society about twenty years ago†. The ſame opinion was before, but only from theoretical principles, hinted at by Mr. De la Lande, and alſo by the late Dr. Wilſon, of Glaſgow‡; and has, ſince the publication of my paper, been taken up by ſeveral aſtronomers§, who agree that ſuch motion exiſts. In conſequence of this, let us now, according to No. 3. aſſign to the ſun a motion in ſpace, of a certain velocity and direction. Admitting therefore α and x to be at reſt, let the angle PαO be 90 degrees; then, by the 1ſt caſe of No. 3, we find that none of the obſerved changes of the angles of poſition will admit of an explanation. There is moreover an evident conceſſion of the point in queſtion, in the very ſuppoſition of the above angle of 90 degrees; for, if x be at the ſame diſtance as α from the ſun, and no more than 5″ from that ſtar, its real diſtance, compared to that of the ſun from the ſtar, will be known; and, ſince that muſt be leſs than the 40 thouſandth part of our diſtance from Caſtor, theſe two ſtars muſt neceſſarily be within the reach of each others attraction, and form a binary ſyſtem.

(d) Let us now take the advantage held out by the 2d caſe of No. 3, which allows us to place x far behind α; in which

* See page 211 of the treatiſe before referred to.

† Philoſ. Tranſ. vol. lxxiii. p. 247.

‡ See my note in Phil. Tranſ. vol. lxxiii. p. 283.

§ See *Aſtronomiſches Jahrbuch für das Jahr* 1786; *ſeite* 259. *Uber die fortruckung unſeres Sonnen-Syſtems, von* Herrn *Profeor* Prevoſt. *Und für das Jahr* 1805; *ſeite* 113.

ſituation

ſituation, the angle PαO will be more than 90 degrees. The ſtar x being leſs than α, renders this hypotheſis the more plauſible. Now, as a motion of Caſtor, be it real or apparent, has actually been aſcertained, we cannot ſet it aſide; the real motion of O, therefore, in order to account for the apparent one of α, muſt be of equal velocity, and in a contrary direction: that is, when decompoſed, 0″,105 towards f, and 0″,12, towards s. The effect of the ſun's moving from O towards f, according to the 1ſt Table in No. 3, is, that the diſtance between the two ſtars will be diminiſhed, and the angle of poſition increaſed. But theſe are both contrary to the obſervations I have given. The motion of O in declination towards s, according to the ſame Table, will ſtill diminiſh the diſtance of the two ſtars, but will alſo diminiſh the angle of poſition. Then, ſince a motion in right aſcenſion increaſes the angle, while that in declination diminiſhes it, the ſmall ſtar may be placed at ſuch a diſtance that the difference in the parallax, ariſing from the ſolar motion, ſhall bring the angle of poſition, in $23\frac{1}{2}$ years, from 32° 47′ to 10° 53′; which will explain the obſerved change of that angle. The diſtance of the ſtar x, for this purpoſe, muſt be above $2\frac{1}{2}$ times as much as that of α from us. But, after having in this manner accounted for the alteration of the angle of poſition, we are, in the next place, to examine the effect which ſuch a difference of parallax muſt produce in the apparent diſtance of the two ſtars from each other. By a graphical method, which is quite ſufficient for our purpoſe, it appears, that the union of the two motions in right aſcenſion and declination, muſt have brought the two ſtars ſo near, as to be only about half a diameter of the large ſtar from each other; or, to expreſs the ſame in meaſures, the centers of the ſtars muſt now be 1″,8 nearer than they were $23\frac{1}{2}$ years ago. But this my obſervations cannot allow; for we have already ſhown, that any change of more than 3 or 4-tenths of a ſecond muſt have been perceived.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

If, on the other hand, we place the ſtar x at ſuch a diſtance that the ſolar parallax may only bring it about 4-tenths of a ſecond nearer to α, which is a quantity we may ſuppoſe to have eſcaped our notice in eſtimating the apparent diſtance of the two ſtars, then will the angle of poſition be above 20 degrees too large. This ſhows, that no diſtance, beyond Caſtor, at which we can place the ſtar, will explain the given obſervations.

Obſervations and inferences reſpecting the changes of relative ſituations in ſtars extremely near each other.

(e) The laſt remaining trial we have to examine, is to ſuppoſe x to be nearer than α; the angle P α O, will then be leſs than 90 degrees; and the effect of a motion of O towards f, by the 2d Table in No. 3, will be an increaſe of the diſtance of the two ſtars, and a diminution of their angle of poſition. But the motion Os, which is alſo to be conſidered, will add to the increaſe of the diſtance and counteract the diminution of the angle. It is therefore to be examined, whether ſuch an increaſe of diſtance as we can allow to have eſcaped obſervation, will explain the change which we know to have happened in the angle, during the laſt $23\frac{1}{2}$ year. By the ſame method of compounding the two motions as before, it immediately appears, than we cannot place the ſmall ſtar more than about 1-tenth of the diſtance Oα on the ſide of Caſtor, without occaſioning ſuch an increaſe of the apparent diſtance of the two ſtars as cannot poſſibly be admitted; and that, even then, the angle of poſition, inſtead of being leſs, will be a few degrees larger, at the end of $23\frac{1}{2}$ years, than it was at the beginning. This hypotheſis, therefore, like all the foregoing ones, muſt alſo be given up, as inconſiſtent with my obſervations.

It is moreover evident, that the obſervations of aſtronomers on the proper motion of the ſtars in general, will not permit us to aſſume the ſolar motion at pleaſure, merely for the ſake of accounting for the changes which have happened in the appearances of a double ſtar. The proper motion of Caſtor, therefore, cannot be intirely aſcribed to a contrary motion of the ſun. For we can aſſign no reaſon why the proper motion of this ſtar alone, in preference, for inſtance, to that of Arcturus, of Sirius, and of many others, ſhould be ſuppoſed to ariſe from a motion of the ſolar ſyſtem. Now, if they are all equally intitled to partake of this motion, we can only admit it in ſuch a direction, and of ſuch a velocity, as will ſatisfy mean direction and velocity of the general proper motions of the ſtars; and place all deviations to the account of a real proper motion in each ſtar ſeparately.

Double Motion.

(f) In order to explain the phenomena of our double ſtar, according to No. 4, by the motion of two bodies, for inſtance α and x, it will be required that they both ſhould move in given

directions; that the velocities of their motions ſhould be in a given ratio to each other; and that this ratio ſhould be compounded with the ratio of their diſtances from O; a ſuppoſition which muſt certainly be highly improbable. To ſhew this with ſufficient evidence, let us admit that, according to the beſt authorities, the annual proper motion of Caſtor is —0″,105 in right aſcenſion, and 0″,12 in declination towards the north. Then, as the ſmall ſtar, without changing its diſtance, has moved through an angle of 21° 54′, the only difference in the two motions of theſe ſtars, will be expreſſed by the extent of the chord of that angle. To produce the required effect, it is therefore neceſſary that the motion of α, which is given, ſhould regulate that of the ſmall ſtar, whoſe relative place at the end of $23\frac{1}{2}$ years is alſo given. Then, as α moves in angle of 53° 31′ north preceding, and with a velocity which, being expreſſed by the ſpace it would deſcribe in $23\frac{1}{2}$ years, will be 3″,51, it is required that x ſhall move in an angle of 29° 25′, likewiſe north preceding, and with a velocity of 3″,02. The ratio of the velocities, therefore, and the directions of the motions, are equally given. But this will not be ſufficient for the purpoſe: their diſtance from O muſt alſo be taken into conſideration. It has been ſhown, that the two ſtars cannot be at an equal diſtance from us, without an evident connection; it will therefore be neceſſary for thoſe who will not allow this connection, to place one of them nearer to us than the other. But, as the motions which have been aſſumed, when ſeen from different diſtances, will ſubtend lines whoſe apparent magnitudes will be in the inverſe ratio of the aſſumed diſtances, it is evident that this ratio, if the motions are given, muſt alſo be a given one; or that, if the diſtances be aſſumed, the ratio of the motions muſt be compounded with the ratio of the diſtances. How then can it be expected that ſuch preciſe conditions ſhould be made good, by a concurrence of circumſtances owing to mere chance? Indeed, if we were inclined to paſs by the difficulties we have conſidered, there is ſtill a point left which cannot be ſet aſide. The motion of the ſolar ſyſtem, although its preciſe direction and velocity may ſtill be unknown, can hardly admit of a doubt; we have therefore a third motion to add to the former two, which conſequently will bring the caſe under the ſtatement contained in our 7th number, and will be conſidered hereafter.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

(g) If

Obſervations and inferences reſpecting the changes of relative ſituations in ſtars extremely near each other.

(g) If we ſhould intend to change our ground, and place the two motions in O and x, it will then be conceded, that the motion of α is only an apparent one, which owes its exiſtence to the real motion of the ſun. By this, the effect of the ſolar parallax on any ſtar at the ſame diſtance will be given; and it cannot be difficult to aſſume a motion in x, which ſhall, with the effect of this given parallax, produce the apparent motion, in the direction of a chord from the firſt to the laſt angle of poſition pointed out by my obſervations; taking care, however, not to place the ſtars α and x at the ſame diſtance from us; uſing the inverſe ratio of the ſolar parallax as a multiple in the aſſigned motion. For inſtance, let the ſun have a motion of the velocity expreſſed as before by 3″,51, and in a direction which makes an angle of 53° 13′ ſouth following with the parallel of α Geminorum; and let the ſmall ſtar x have a real motion in an angle of 18° 40′ ſouth preceding from the parallel of its ſituation, and with a real velocity which, were it at the diſtance of α, would carry it through 8″,89. Then, if the diſtance of the ſmall ſtar be to that of the large one as 3 to 2, the effect of the ſolar parallax upon it will be $\frac{2}{3}$ of its effect upon α; that is, while α, which is at reſt, appears to move over a ſpace of 3″,51, in an angle of 53° 31′ north preceding, the parallactic change of place in x will be 2″,34 in the ſame direction. This, though only an apparent motion, will be compounded with the real motion we have aſſigned to it, but which, at the diſtance of α, will only appear as 1″,26; and the joint effect of both will bring the ſtar from the place in which it was ſeen $23\frac{1}{2}$ years ago, to that where now we find it ſituated. α, in the ſame time, will appear to have had an annual proper motion of — 0″,105 in right aſcenſion, and 0″,12 in declination towards the north; and thus all phenomena will be explained.

From this ſtatement, we may draw a conſequence of conſiderable importance. If we ſucceed, in this manner, in accounting for the changes obſerved in the relative ſituation of the two ſtars of a double ſtar, we ſhall fail in proving them to form a binary ſyſtem; but, in lieu of it, we ſhall gain two other points, of equal value to aſtronomers. For, as α Geminorum, according to the foregoing hypotheſis, is a ſtar that has no real motion, its apparent motion will give us the velocity and direction of the motion of the ſolar ſyſtem; and, this being

being obtained, we ſhall alſo have the relative parallax of every ſtar, not having a proper motion, which is affected by the ſolar motion. Aſtronomical obſervations on the proper motion of many different ſtars, however, will not allow us to account for the motion of α Geminorum in the manner which the foregoing inſtance requires; the hypotheſis, therefore, of its being at reſt, muſt be rejected.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

(*h*) If we place our two motions in O and α, we ſhall be led to the ſame concluſion as in the laſt hypotheſis. The known proper motion of α, and the ſituations of the ſmall ſtar in 1779 and 1803, given by my obſervations, will aſcertain the apparent motion of x, now ſuppoſed to be at reſt. Then, ſince the change in the place of x muſt be intirely owing to the effect of parallax, it will conſequently give us, in the ſame manner as before, the quantity and direction of the motion of the ſolar ſyſtem, and the relative diſtances of all ſuch ſtars as are affected by it. But, here again, the ſolar motion required for the purpoſe is ſuch as cannot be admitted; and the hypotheſis is not maintainable.

Motion of the three Bodies.

(*i*) There is now but one caſe more to conſider, which is, according to No. 5, to aſſign real motions to all our three bodies; and this may be done as follows. Suppoſe the ſun to move towards λ Herculis, with the annual velocity 1.

Let the apparent motion of α Geminorum be as it is ſtated in the aſtronomical tables before mentioned; but ſuppoſe it to ariſe from a compoſition of its real motion with the effect of the ſyſtematical parallax, as we may call that apparent change of place of ſtars which is owing to the motion of the ſolar ſyſtem. Let the real motion of x, aided by the effect of the ſame parallax, be the cauſe of the changes in the angle of poſition which my obſervations have given. We may admit the largeſt of the two ſtars of our double ſtar to be of the ſecond magnitude; and, as we are not to place x too near α, we may ſuppoſe its diſtance from O to be to that of α from the ſame as 3 to 2. In this caſe, O will move from the parallel of α, in an angle of 60° 37′ north following, with an apparent annual velocity of ,4536. The motion of α in right aſcenſion, may be intirely aſcribed to ſolar parallax; but its change of declination, cannot be accounted for in the ſame manner. Let us therefore

Obſervations and inferences reſpecting the changes of relative ſituations in ſtars extremely near each other.

admit that the ſolar velocity, in the direction we have calculated, will produce an apparent retrogate motion in α, which, in $23\frac{1}{2}$ years will amount to 2″,085 in right aſcenſion. But the ſame parallax will alſo occaſion a change in declination, towards the ſouth preceding, of 3″,701 ; and, as this will not agree with the obſerved motion of α, we muſt account for it by a proper motion of this ſtar directly towards the north. The real annual velocity required for this purpoſe, muſt be 1,3925.

The apparent motion of x, by parallax, at the diſtance we have placed this ſtar, will be 2″,832 towards the ſouth preceding ; and, by aſſigning to it an annual proper motion of the velocity 1,3354, in the direction of 73° 10′ north preceding its own parallel, the effect of the ſolar parallax and this proper motion together, will have cauſed the ſmall ſtar, in appearance, to revolve round α, ſo as to have produced all the changes in the angle of poſition which my obſervations have given ; and, at the ſame time, α will have been ſeen to move from its former place, at the annual rate of 0″,105 in right aſcenſion, and 0″,12 in declination towards the north.

In this manner, we may certainly account for the phenomena of the changes which have taken place with the two ſtars of α Geminorum. But the complicated requiſites of the motions which have been expoſed to our view, muſt ſurely compel every one who conſiders them to acknowledge, that ſuch a combination of circumſtances involves the higheſt degree of improbability in the accompliſhment of its conditions. On the other hand, when a moſt ſimple and ſatisfactory explanation of the ſame phenomena may be had by the effects of mutual attraction, which will ſupport the moving bodies in a permanent ſyſtem of revolution round a common centre of gravity, while at the ſame time they follow the direction of a proper motion which this centre may have in ſpace, it will hardly be poſſible to entertain a doubt to which hypotheſis we ought to give the preference.

As I have now allowed, and even ſhown, the poſſibility that the phenomena of the double ſtar Caſtor may be explained by proper motions, it will appear that, notwithſtanding my foregoing arguments in favour of binary ſyſtems, it was neceſſary, on a former occaſion, to expreſs myſelf in a conditional manner,*

ner,* when, after having announced the contents of this Paper, I added, "*ſhould theſe obſervations be found ſufficiently concluſive;*" for, if there ſhould be aſtronomers who would rather explain the phenomena of a ſmall ſtar appearing to revolve round Caſtor by the hypotheſis we have laſt examined, they may certainly claim the right of aſſenting to what appears to them moſt probable.

(To be continued.)

XII.

On the Nature of the Varieties of Engrafted Fruit-Trees, with a Plan for increaſing the Number of New valuable Fruits. By T. S. Dyot Bucknall, *Eſq. M. P* †.

To Mr. CHARLES TAYLOR.

SIR,

Introduction.

SOME friends have requeſted that I would introduce another Paper on the Nature of the valuable Varieties of engrafted Fruits, as they are of opinion that the Eſſay in the 17th Volume of the Tranſactions of the Society is not ſufficiently extended for a ſubject ſo important to the Fruit-growers, and thoſe intereſted in the productions of Fruits. As a proof of my willingneſs to make the Orchardiſt as perfect as I can, I beg you to preſent my compliments to the Society, with the following elucidations.

Importance.

This is a ſubject in rural oeconomy which ought to be much better underſtood than it is, in order to enable the planters to judge of the ſorts proper to be planted, either as an article of pleaſure, profit, or recreation; as much of the credit of the plantation muſt ariſe from judiciouſly chooſing Trees of the beſt, new, or middled-aged ſorts, and not of the old worn-out varieties, which latter cannot, in the planting of Orchards in common ſituations, ever form *valuable Trees*, and muſt end in the diſappointment of the planter.

Grafted fruits are not permanent.

Engrafted Fruits, I have before ſaid, and I now repeat, are not permanent. Every one of the leaſt reflection muſt ſee that

* See Phil. Trans. for 1802, page 486.

† Society of Arts, xx. 144.

there is an eſſential difference between the power and energy of a ſeedling plant, and the tree which is to be raiſed from cuttings or elongations. The ſeedling is endued with the energies of nature, while the graft, or ſcion, is nothing more than a regular elongation, carried perhaps through the ſeveral repeatings of the ſame variety; whereas the ſeed, from having been placed in the earth, germinates and becomes a new plant, wherever nature permits like to produce like in vegetation; as in the oak, beech, and other maſt-bearing trees. Theſe latter trees, from each paſſing through the ſtate of ſeedlings, are perfectly continued, and endued with the functions of forming perfect ſeeds for raiſing other plants by evolution, to the continuance of the like ſpecies.

The varieties continued by grafting laſt only for a time.

This is not the caſe with engrafted fruits. They are doomed by nature to continue for a time, and then gradually decline, till at laſt the variety is totally loſt, and ſoon forgotten, unleſs recorded by tradition, or in old publications.

Reaſon, with which Providence has moſt bountifully bleſſed ſome of our ſpecies, has enabled us, when we find a ſuperior variety, to engraft it on a wilding ſtock, or to raiſe plants from layers and cuttings, or even to raiſe up the roots, and thus to multiply our ſources of comfort and pleaſure. This, however, does not imply that the multiplication of the ſame variety, for it is no more, ſhould laſt for ever, unleſs the ſpecies will naturally ariſe from ſeed.

Proceſs of raiſing fruits, and increaſing them by grafting.

Nature, in her teaching, ſpeaks in very intelligible language, which language is conveyed by experience and obſervation. Thus we ſee that among promiſcuous ſeeds of fruits of the ſame ſort, one or more may ariſe, whoſe fruits ſhall be found to poſſeſs a value far ſuperior to the reſt in many diſtinguiſhable properties. From experience, alſo, we have obtained the power, by engrafting, of increaſing the number of this newly-acquired tree, can change its country, give it to a friend, ſend it beyond the ſeas, or fill a kingdom with that fruit, if the natives are diſpoſed ſo to do. Thus we ſeem to have a kind of creative power in our own hands.

From the attention lately paid to the culture of engrafted Fruits, I hope we are now enabled to continue a ſuppoſed happily acquired tree, when we can find it, for a much longer duration than if ſuch variety had been left in the ſtate of unaſſiſted nature; perhaps I may ſay for a duration as long again,

or

or ſomething more. After theſe ſanguine expectations, I may reaſonably be aſked, to what does all this amount? for here there is no direct permanency—and why? The *why* is very obvious—becauſe the kernels within the fruit, which are the ſeed of the plants for forming the next generation of trees, will not produce their like. I allow they may do ſo accidentally; but nothing more can be depended on.

Proceſs of raiſing fruits and increaſing them by grafting.

For example, ſuppoſe we take ten kernels or pips of any apple raiſed on an engrafted ſtock: ſow them, and they will produce ten different varieties, no two of which will be alike; nor will either of them cloſely reſemble the fruit from whence the ſeeds were collected. The leaves alſo of thoſe trees raiſed from the ſame primogenious or parent ſtock, will not *actually* be a copy of the leaves of any one of the varieties or family, to which each is connected by a vegetable conſanguinity. I intentionally uſed the word *actually*, becauſe a reſemblance may be found, though not much of that is to be expected.

I beg that what has been laſt mentioned may not be taken as a diſcouragement to attempts for raiſing new varieties. I was obliged to ſpeak very ſtrongly, in order to place the culture upon its true foundation. I think it need not be obſerved, that there is no acquiring a new variety, but through the means of a ſeedling plant; and therefore whoever wiſhes to ſucceed muſt attempt it that way, or wait till others in their plantations may more fortunately produce it.

In chooſing the ſeeds, that apple is moſt likely to produce the cleareſt and fineſt plants, whoſe kernels are firm, large, and well ripened. The ſize of the fruit is not to be regarded; for large apples do not always ripen their fruit well, or rather for cider the ſmall fruits are generally preferred for making the ſtrongeſt, higheſt-flavoured liquor. And from what I have been able to collect in the cider-countries, it is there the opinion, that an apple ſomething above the improved crab promiſes the beſt ſucceſs. This advantage alſo attends the practice: if there are no valuable apples raiſed from that attempt, theſe wildings will make excellent ſtocks to engraft upon.

Gentlemen who actually employ themſelves in attempting to acquire new varieties, ſhould remember that they ought to ſelect all the ſets, from the bed of apple-quick, whoſe appearance is in the leaſt degree promiſing, and plant them together, at ſuch a diſtance as to allow each to produce its fruit, which will

will happen in about twelve, fifteen, or eighteen years. My friend Mr. Knight, who undoubtedly is the firſt in actual exertions for procuring theſe happily acquired new varieties, has had two plants bear fruit at ſix years old, and one at five. The cider-countries have offered ſeveral premiums for procuring new varieties, and ſome with good effect. Premiums have been given both to Mr. Knight and Mr. Alban.

When the new variety is to be raiſed from a valuable *admired* apple, I ſhould recommend the placing theſe ſeeds in a garden-pot, filled with mould from an old melon-bed; carrying the pot into a retired ſituation near the water, and giving attention to run the plants to as large a ſize as is convenient within eighteen months. With this view, the pot ſhould be placed in the green-houſe the firſt winter; and when the plants are afterwards to be ſet out in the ſpots, they ſhould not be placed under the drip of trees, or much expoſed to the winds.

Two inſtances have been mentioned, the improved crab, and moſt admired apple; but prudence ſays, try all ſorts, and ſomething probably will ariſe; and the proceſs is attended with little trouble or expence to a perſon who conſtantly reſides in the country: yet, after all this ſcientific care, the apple may want flavour, and be in other reſpects nothing better than a common wilding.

It is an undoubted fact, and worthy of obſervation, that all the different trees of the ſame variety have a wonderful tendency to ſimilarity of appearance among themſelves; and that the parent ſtock, and all engrafted from it, have a far greater reſemblance to each other, that can be found in any part of the animal creation; and this habit does not vary to any extent of age.

Facts reſpecting engrafted fruits, their degree of permanence, &c.

As an encouragement in attempting to increaſe the number of new valuable fruits, we can prove that the golden pippin is native Engliſh. The red-ſtreak, a ſeedling of Hereford-ſhire, if not raiſed, yet was firſt brought into notice by Lord Scudamore, and was for a long time called Scudamore's Crab. The Stire Apple was accidentally raiſed in the Foreſt of Dean, in Glouceſterſhire, and took the name of *Foreſt Stire*. The cider made from this apple was the ſtrongeſt the country ever produced, according to any living record. The Haglo-crab, the beſt cider fruit now remaining, was diſcovered

Facts reſpecting engrafted fruits, their degree of permanence, &c.

m the pariſh of Ecloe, on the banks of the Severn; and, about ſixty or ſeventy years ago, many ſcions were taken from this tree by Mr. Bellamy, and engrafted on ſeedling ſtocks about Roſs. Theſe are now grown old; and, to aſcertain the age of the variety, I went with Charles Edwin, Eſq. to Ecloes, in hopes of ſeeing the primogenious of this family. The proprietor of the eſtate acquainted Mr. Edwin that it had ceaſed to bear years ago, and was cut down. Thoſe at Roſs are but poor bearers now, and I ſhould ſuppoſe the variety muſt be 140 years old, though Marſhall, who wrote in the year 1786, mentions theſe trees were prolific, and he ſuppoſes the ſort to be about eighty years old; but, from preſent experience, it muſt be much more. The Tinton Squaſh-pear is of Glouceſterſhire; the Barland and Old-field were near Ledbury, Herefordſhire. The two laſt pears clearly bear the names of the two fields where they were raiſed. The Barland fell about ſix years ago, viſibly from weight and longevity, which was ſuppoſed to have been about 200 years. There had been many other names of eſtimation handed down to us, though the realities are now totally worn out, and have ceaſed to exiſt. Can any better proof be deſired, that engrafted fruits are not permanent, then the regret we feel for the loſs of theſe old valuable fruits.

To make my paper as ſhort as convenient, I have dwelt only on the apple and pear; yet all the engrafted fruits are under the ſame predicament of the ſeed not producing its like, and the offspring in time falling into a nothingneſs of growth and bearing, though that ſpace of time muſt certainly depend on the natural longevity and hardneſs of the ſort, ſoil, poſition, care, &c. All theſe are more fully expreſſed in the papers publiſhed in the different volumes of the Tranſactions of this Society, and the two volumes of the Orchardiſt, wherein the whole ſyſtem is extended, to form a rational culture for the management of Standard Fruits.

It ſhould be remembered, that as I am now alluding to the ſtate of actual permanency, fifty years are to be accounted as nothing; and as often as we come to that point, we are compelled to reſort to our firſt aſſertion, "That engrafted fruits are not permanent, they being continued from elongations, and not raiſed as a repetition of ſeeds." This is the only rational

Facts reſpecting engrafted fruits, their degree of permanence, &c.

rational way as yet introduced of accounting for the loſs of the valuable old varieties of fruits. Should a better ſyſtem be introduced, I ſhall readily adopt it; but this ſufficiently anſwers the purpoſes of the planter.

Some years ago, from due inveſtigation and thorough conviction, I propagated this principle; and it was publiſhed in the 17th volume of the Society's Tranſactions, in the following words: "All the grafts taken from this firſt tree, or parent ſtock, or any of the deſcendants, will for ſome generations thrive; but when this firſt ſtock ſhall, by mere dint of old-age, fall into actual decay, a nihility of vegetation—the deſcendants, however young, or in whatever ſituation they may be, will gradually decline; and, from that time, it would be imprudent, in point of profit, to attempt propagating that variety from any of them. This is the dogma which muſt be received. I do not expect a direct aſſent, neither do I wiſh it, for it ſhould be taken with much reſerve; but it is undoubtedly true." Theſe conſiderations ſhould ſtimulate us in ſearching after new varieties, equal, or perhaps ſuperior to thoſe of which we regret the loſs.

Obſerve that, from the time the kernel germinates for apple-quick, ſhould the plant be diſpoſed to form a valuable variety, there will appear a regular progreſſive change, or improvement, in the organization of the leaves, until that variety has ſtood, and grown ſufficient to bloſſom and come into full bearing; that is, from the ſtate of infancy to maturity; and it is this and other circumſtances, by which the inquiſitive eye is enabled to form the ſelection among thoſe appearing likely to become valuable fruits. But from that time the new variety, or ſelected plant, compared with all the engraftments which may be taken from it, or any of them, theſe ſhall ſhew a moſt undeviating ſameneſs among themſelves.

It is readily allowed, that the different varieties of fruits are eaſily diſtinguiſhed from each other by many particulars, not only reſpecting their general fertility, and the form, ſize, ſhape, and flavour of the fruit, but alſo the manner of the growth of the tree, the thickneſs and proportion of the twigs, their ſhooting from their parent ſtem, the form, colour, and conſiſtence of the leaf, and many other circumſtances, by which the variety can be identified; and were it poſſible to

engraft

engraft each variety upon the ſame ſtock, they would ſtill retain their diſcriminating qualities, with the moſt undeviating certainty.

Facts reſpecting engrafted fruits, their degree of permanence, &c.

The proper concluſion to be drawn from the ſtatement in the laſt paragraph, is this—that were any one to put the thought in practice on a full-grown hardy or crab ſtock, it would produce an excellent proof that engrafted fruits are not permanent. For if twenty different varieties were placed together, ſo that each might receive its nurture from the ſame ſtem, they would gradually die off in actual ſucceſſion, according to the age or ſtate of health of the reſpective variety, at the time the ſcions were placed in the ſtock; and a diſcriminating eye, uſed to this buſineſs, would nearly be able to foretell the order in which each ſcion would actually decline. Should it alſo happen that two or three ſuckers from the wilding ſtock had been permitted to grow among the *twenty grafts*, ſuch ſuckers or wilding ſhoots will continue, and make a tree after all the reſt are gone. A further conſequence would reſult from the experiment: among ſuch a number of varieties, each of the free growers would ſtarve the delicate, and drive them out of exiſtence only ſo much the ſooner. It muſt be obſerved, that this ſuppoſed ſtem is the foſter-parent to the twenty ſcions, and real parent to the ſuckers; and thoſe the leaſt converſant with engrafted fruits know the advantage acquired from this circumſtance. And here it is worth while remarking, that a Gaſcoyne, or wild cherry, will grow to twice the ſize that ever an engrafted cherry did.

By an experiment we have had in hand for five years, it will appear that the roots and ſtem of a large tree, after the firſt ſet of ſcions are exhauſted or worn out, may carry another ſet for many years; and we ſuſpect a third ſet, provided the engrafting is properly done, and the engrafter chooſes a new variety. Now the Ripſton pippin, of Yorkſhire, is the favourite, as being a free grower and good bearer, with fine fruit. This however may be certainly depended on, that when a new apple is raiſed from ſeed, if a ſcion were placed in a retired ſituation, and conſtantly cut down, as a ſtool in a copſe-wood, and the apple never ſuffered to fulfil the intentions of nature in bearing fruit, the practitioners of the following ages may ſecure ſcions from that ſtool, to continue the variety much longer. Hence, though I have written as much as is in

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Facts reſpecting engrafted fruits, their degree of permanence, &c.

my power againſt permanency, yet I have taken ſome pains to aſſure the planters, that forecaſt, ſelection, pruning, cleanlineſs, and care, will make the orchards turn to more profit for the riſing generations, than what they have done for the laſt hundred years.

To place the nature of varieties in its true light, for the information of the public, I muſt maintain, that the different varieties of the apple will, after a certain time, decline, and actually die away, and each variety, or all of the ſame ſtem or family, will loſe their exiſtence in vegetation; and yet it is a known fact, and mentioned in the 17th volume of the Tranſactions, that after the debility of age has actually taken poſſeſſion of any variety, it will yet thrive by being placed againſt a ſouthern wall, and treated as wall-fruit. Who, however, can afford to raiſe cider at that expenſe, except as matter of curioſity, to prove, that when the vital principle in vegetation is nearly exhauſted, a ſuperior care and warmth will ſtill keep the variety in exiſtence ſome time longer?

It ſhould be underſtood, that the external air of Britain is rather too cold for the delicate fruits, which is the reaſon why, in the Orchardiſt, I lay ſuch a ſtreſs on procuring *warmth* for the trees, by *draining*, *ſhelter*, and *manure*. It would be now loſt time to attempt to recover the old varieties as an article of profit.

If I have not expreſſed myſelf, in this Eſſay on the Nature of Varieties, with ſo much clearneſs and conviction as might have been expected, it ſhould be conſidered that it is an abſtruſe ſubject, very little underſtood, and requiring at firſt ſome degree of *faith*, *obſervation*, and *perſeverance*. The prejudices of mankind revolt againſt it. They are not diſpoſed to allow the diſtinction of nature; and they imagine, that in the act of engrafting or multiplying they give new life, whereas it is only continuing the exiſtence of the ſame tree, ſtick, or bud. Obſerve what I ſaid before:—the ſeed of the apple, when placed in the earth, germinates, and unfolds itſelf into a new plant, which ſucceſſively paſſes through the ſtages of infancy, maturity, and decay, like its predeceſſors. I might ſay, all created nature is ſimilar in this reſpect; though, from the circumſtance that varieties are much longer-lived than man, the plants have appeared to be poſſeſſed of

eternal

eternal powers of duration: nothing ſublunary, however, which poſſeſſes either animal or vegetable life, is exempt from age and death.

Facts reſpecting engrafted fruits, their degree of permanence, &c.

Within the laſt twenty years I have travelled many hundred miles, and converſed with the moſt intelligent men in each country; and I now want to convince mankind, for no other reaſon than becauſe it is their intereſt ſo to believe, that there is in creation an order of beings (engrafted fruits) ſo formed, that we have the power of multiplying a ſingle variety, to whatever number of trees we pleaſe;—that the firſt ſet ariſes from a ſmall ſeed;—that the next and deſcendent ſets are propagated by engraftings, or from cuttings, layers, &c.;—and that although theſe trees may amount to millions, yet, on the death of the primogenious or parent ſtock, merely from old-age, or nihility of growth, each individual ſhall decline, in whatever country they may be, or however endued with youth and health. I ſay they ſhall gradually begin to decline; and in the courſe of time, or of centuries, to thoſe who would prefer that expreſſion, the *whole variety* will ſcarcely have a ſingle tree remaining to ſhow what the fruit was. Let thoſe who are not diſpoſed to aſſent to this ſtatement, aſk themſelves what is become of the old loſt varieties? did they die, or did wicked men maliciouſly cut them up?

I, who am firmly convinced of the truth of what I have advanced on this ſubject, have no doubt but that the ſame would happen by engrafting on the Oak or Beech, if the maſt raiſed from the engrafted tree did not produce the like; for there the queſtion turns.

Is it not known, that the woodman, in ſetting out his ſapling oaks, always ſelects new ſeedling plants, and never continues one upon an old ſtool; and that if he ſhould ſo blunder, that tree, from the ſtool, will neither have the freedom of growth, nor the ſize or firmneſs of timber, equal to a new-raiſed plant.

I wiſh I could perſuade my friends, that, with the ſame attention with which the woodman acts, the planter is to raiſe his orchard from the young fruits which thrive in the neighbourhood, or are in health and full bearing in the country whence they are to be brought.

The fruit-grower ſhould look to ſelection, cleanlineſs, and care. To me it is a circumſtance perfectly indifferent, whether

Facts reſpecting engrafted fruits, their degree of permanence, &c.

ther he is to uſe Mr. Forſyth's compoſition, Mr. Bulingham's boiled linſeed oil, or my medication. I only maintain that the wounded parts of trees want ſomething to deſtroy the inſects and vermin, and heal the wood, from which the trees are kept in health.

Let thoſe who are bleſſed with fruit-plantations attend to their preſervation, and not leave them to the ſtate of unaſſiſted nature.

I am, Sir,
Your moſt obedient Servant,
THO. SKIP DYOT BUCKNALL.

Hampton-Court,
12th Oct. 1801.

XIII.

Explanation of the Subjects concerning which Queſtions were propoſed at Page 71 *of the preſent Volume; namely,* 1. *The Place of the Erect Image behind a Concave Speculum;* 2. *Of the Image formed by a Concavo Convex Mirror, which is not Left-handed, and has the Property of revolving along with the Mirror; and* 3. *The Figure of the Sky. By* W. N.

Anſwers to certain queſtions.

AS the queſtions of my correſpondent R. B. have not been treated by any other of my friends, I ſhall here diſcuſs them according to my promiſe.

Erect image in a concave mirror.

1. The queſtion reſpecting the erect image in the concave mirror relates to a phenomenon which formed the ſubject of diſcuſſion in print very early in the laſt century, though I cannot at this moment recollect the authors who have related the facts and arguments. R. B. is perfectly correct as to the various divergence of the rays compoſing the pencils which iſſue from radiant points at different diſtances from the eye, but it does not appear that our notions of diſtance are greatly regulated by that circumſtance. Our judgment of the diſtances of near objects is partly founded on the adjuſtment required to be made in the eye itſelf to produce diſtinct viſion by the rays of the ſeveral pencils, as R. B. remarks, but much more on a circumſtance commonly noticed by authors, but by him overlooked, namely, the degree of convergence between the optical axes of the two eyes which is required to avoid ſquinting,

Our judgment of diſtance is not founded on the nature of the pencils of light;

or

or the phenomenon of two images. In proof of this, if he will take two pens or pencils or other pointed objects, and, ſhutting one eye, endeavour to apply their points end-wiſe to each other by moving them acroſs the line of ſight, he will find how much leſs preciſely one eye can give a judgment on diſtances, than both eyes. And our judgment of diſtant objects is eſtabliſhed not at all upon the nature of the pencils, but on the perſpective arrangement or angular magnitude of the objects together with their gradual obſcuration (from the interpoſed maſs of air) called aerial perſpective. Hence we find that the ſeveral parts of a well drawn ſketch of mere lines, and ſtill more of an excellent painting, give all the notions of diſtance, though there cannot be any difference in the divergence of the pencils of light to correſpond with that effect. The conſciouſneſs that a body is approaching or retiring ariſes little if at all from any change in the pencils; in near objects that notion is rather gained from the required change in the optical axes, and in theſe and all diſtant objects it is almoſt entirely produced by the angular magnitude becoming larger or ſmaller, while we contemplate the appearances.

but on the ſituation of the eye, and the perſpective.

Theſe truths will explain the image in the concave mirror: For 1. When the face is near the mirror, the image is ſeen by a great convergence of the optical axes, which becomes leſs and leſs as the obſerver retires, at the ſame time that the image itſelf ſubtends a leſs and leſs angle:—It therefore, for both reaſons, appears to retire. 2. At a greater diſtance the image ſeems ſtationary while the obſerver retires, becauſe the angular enlargement of the image is nearly compenſated by its diminution from increaſed diſtance; and 3. At a ſtill greater diſtance the angular enlargement increaſes ſo faſt that the image ſeems rapidly to come forward until it is loſt in confuſion.—If both eyes be kept open, the obſerver has a conſiderably accurate notion of the real diſtance of the frame, which, together with the confuſion of two different images preſented to the eyes, may lead him to have the ſentiment of a mere angular enlargement; but if one eye be covered, the notions of retreat, ſtationary poſition, and rapid advance of the image will infallibly be adopted.

The image in a concave mirror ſeems to retire and advance from theſe cauſes.

2. With regard to the concavo-convex mirror, none of the pencils of light are brought truly to a focus. If we conceive the ſurface of the mirror to be divided into zones by lines

Explanation of the effects of a concavo-convex mirror.

drawn perpendicularly acroſs the ſhorteſt arc of convexity that paſſes through the vertex; and theſe lines to be indefinitely near each other;—or to ſpeak leſs mathematically, if the mirror were divided into ſmall parallel ſtripes, drawn right acroſs its concavity;—each of thoſe ſmall zones or ſtripes will reflect the light of any diſtant object to a focus before the mirror without any ſenſible error; whence it will diverge in a kind of flat pencil; and theſe pencils themſelves will not be parallel to each other, but will diverge from a point behind the mirror or virtual focus; ſo that this laſt divergence of the pencils will be at right angles to the divergence of the rays that compoſe them. If therefore we attend to the portion of light which enters the eye from what may be taken to be a radiant point of the image, in this caſe, we ſhall perceive that they cannot be brought to a correct focus on the retina. For if the eye be adjuſted to the place of convergence before the mirror, the focal ſpot will be elongated in the direction of the arc of convexity; and on the contrary, if the eye be adjuſted to the virtual focus of the convex, or point of divergence, the rays of the pencils will not be duly collected, but will render the ſpot on the retina oblong in the direction of the arc of concavity. Nevertheleſs, if the diſtance of the eye be ſo great, as that the difference of adjuſtment for theſe two points ſhall not produce any ſenſible effect of this kind, the whole image will appear ſufficiently neat and diſtinct, though a little deformed; that is to ſay, thoſe dimenſions which are governed by the concavity, will be ſomewhat greater than thoſe governed by the convexity; becauſe as we may affirm, the concave mirror is nearer the eye than the convex. From this general explanation, it will be eaſily underſtood that the image muſt diſappear and become utterly confuſed by the quantity of croſs aberation, when any attempt is made to examine it with a convex lens; and that it will be impoſſible to form an image of the ſun or of any other object upon the ſurface of paper or other ſimilar material. In fact the reflected light from the ſun conſidered as a luminous point, will converge before the mirror to a line or cauſtic curve of which I have not yet conſidered the properties; but which, if the concave mirror were cylindrical, would be a right line at the diſtance of half the radius from the vertex.

It is not neceſſary after what has been ſaid to enter into any fuller explanation of the reaſon why the image is inverted, (that is to ſay either right handed or upſide-down) with regard

to

to the relative poſition of its parts conſidered acroſs the concavity, and the contrary along the lines of convexity;—or, while theſe lines have their poſition altered by rotation of the mirror, the image itſelf will alſo appear to revolve with twice the velocity. Theſe effects, as well as that of the extreme diſtortion of the image, ſeen when the object is within the centre of concavity, will be eaſily apprehended by the reader, whoſe notions of optics are ſufficient to enable him to underſtand the preceding paragraph.

Why the ſky appears flat, and the ſun and moon large in the horizon.

3. The enlarged appearance of the ſun and moon at low altitudes or near the horizon, and the apparent flatneſs of the concavity of the ſky have been explained, as my correſpondent obſerves, by reference to the diminiſhed light of the heavenly bodies, when it paſſes through a long portion of the atmoſphere, and likewiſe to the notions of diſtance which are obtained by looking over a long row of terreſtrial objects. That is to ſay the explanation is grounded on the ſuppoſition or notion of greater diſtance along the horizontal line deduced from conſideration of the aerial and the geometrical perſpective. The obſervations and objections of R. B. appear to me to be perfectly well founded. I think the effect is produced almoſt entirely by the geometrical perſpective, not of the houſes, trees, and other land objects, but of the clouds themſelves. When the clouds are in low diſtinct flakes with clear openings between them, the angular magnitude both of the clouds and their intervals will be greater near the zenith, and will diminiſh as the zenith diſtance encreaſes, ſo as on ſome occaſions to exhibit the appearance of objects running out to an immenſe diſtance in the concavity of an extremely flat dome. On the contrary, when lofty towering maſſes of clouds riſe from the horizon to a conſiderable elevation facing the ſetting ſun, or otherwiſe ſo circumſtanced that the tint of the whole maſs ſhall be very little varied, the mind, ſo far from adopting the notion of an extended dome in that part, ſhall receive the impreſſion of an immenſely elevated wall, with very little curvature as it riſes. Both theſe conditions are not unfrequently to be ſeen at one and the ſame time, and the ſky is then, according to the obſervation of R. B. very far from exhibiting a figure of regular dimenſions. A great variety of intermediate forms and groups of the clouds often preſent themſelves, by which the apparent figure of the ſky will be made to differ from theſe extreme caſes. When the ſky is perfectly and cloſely clouded, we have

have perhaps very little notion of an apparent figure unleſs from our former habits, or from the angular motion produced by the wind. This will be greateſt near the zenith, and by ſhewing us that the clouds over our heads are nearer than the others, will give us a conviction of the figure of a flat arch.

Uſeful Notices reſpecting various Objects. By a Correſpondent.

Cotton in Ink. R. B.

Cotton in ink recommended.

THE ancient practice of putting cotton in ink is almoſt entirely given up. But when we conſider that the colouring matter of ink is merely a precipitate, in the act of ſlow ſubſidence, and that the gum as well as the Gallic acid, are ſubject to ſpeedy decompoſition or mouldineſs by expoſure to the air, we ſhall ſee good reaſons for reſuming it. The black feçula is kept ſuſpended by the cotton; the fluid is prevented from circulating by heat, cold or mechanical agitation, and conſequently preſents a very ſmall ſurface to the air; and laſtly the method of dipping tends to keep the pen clean and in good condition. I can take upon me to aſſert that ink in cotton is blacker, more durable, and much leſs liable to become mouldy. The latter effect may be almoſt entirely prevented by occaſionally turning the cotton upſide down, that is to ſay, every two or three days

Shaving with Water.

Shaving with water.

IN ſome of your early numbers the operation of ſhaving has been rather amply diſcuſſed by yourſelf and correſpondents. I have nothing to add in the way of reaſoning to what has been there brought forward; but I take this opportunity of noticing a fact. Long ago it was obſerved to me that the ſoap anſwers no other purpoſe than that of clearing the ſkin, in proof of which it was aſſerted that the face ſo cleaned may be ſhaved quite as well without the lather as with it. I made trial of this proceſs, but did not find it ſucceed, becauſe I wiped the ſkin dry. But I have ſince found that if the beard be well waſhed with ſoap, and then with clear water, the operation of ſhaving may be very effectually and pleaſantly performed, while the water continues upon the ſkin. The effects are that the razor cuts more keenly and cloſely, and the ſkin ſeems to be much leſs tender. I leave your other correſpondents to reaſon and diſcover the cauſe of the excellence of this method; in the mean time I ſhall continue to uſe it, unleſs a ſtill more conſiderable improvement ſhould be made.

Digester in which the Heat, and pressure, are rendered stationary.

By Sir A. N. Edelcrantz.

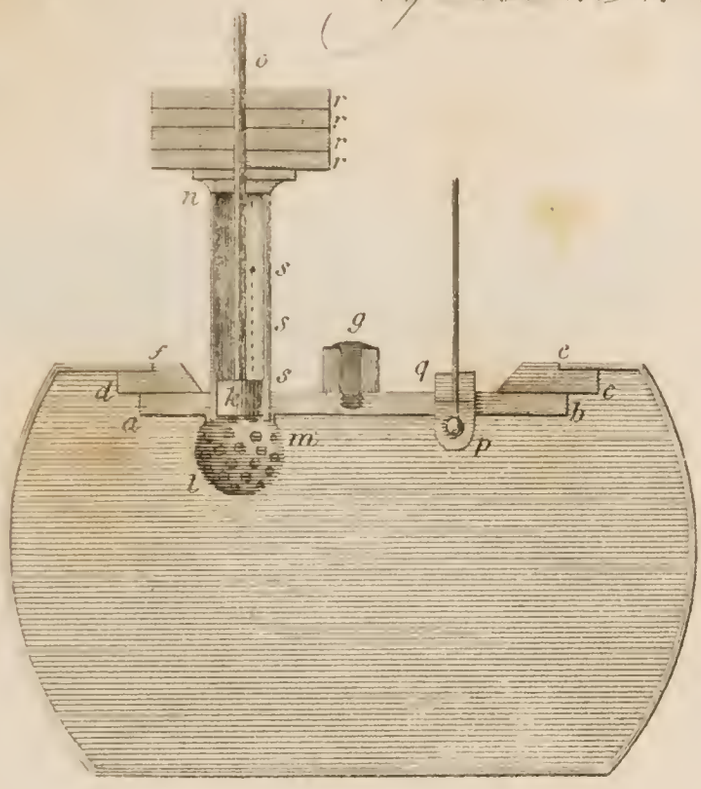


Fig. 1.

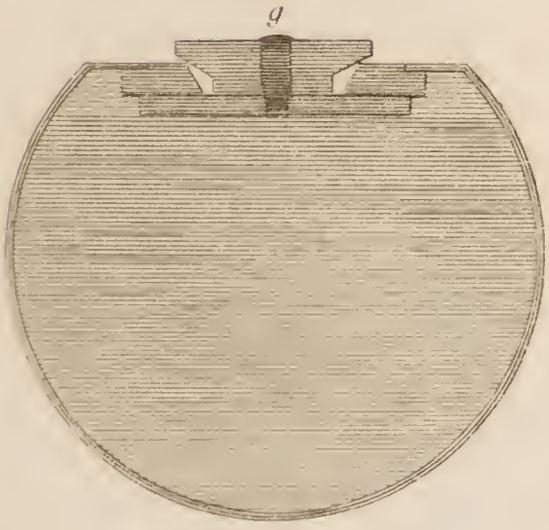


Fig. 2.

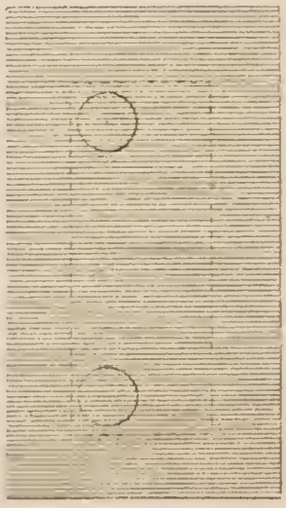


Fig. 3.

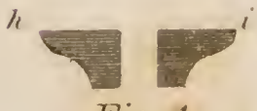


Fig. 4.

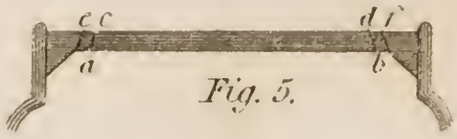
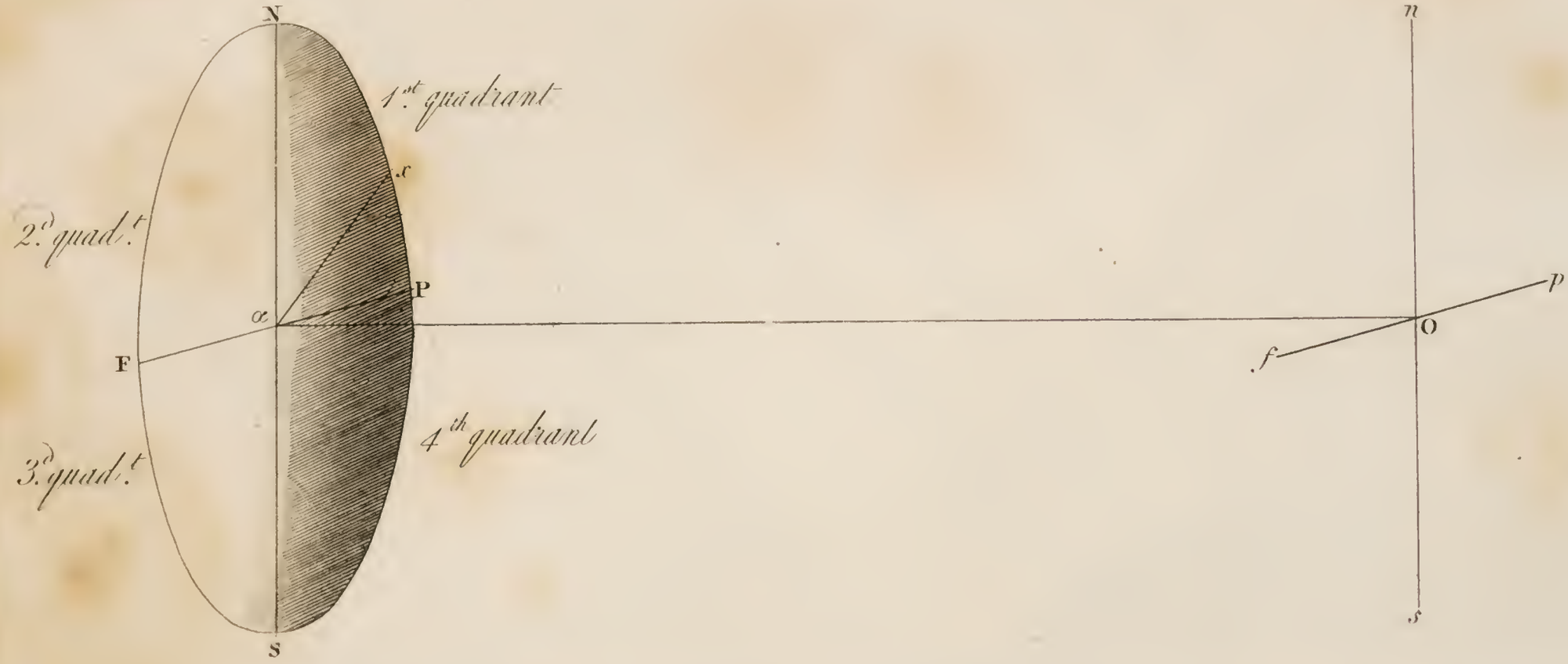


Fig. 5.

Philos. Journal. Vol. VII. Pl. X. p. 240.

Dr. Herschel's theory of the Changes in the situation of double Stars.



Mutlow Sc. Rupsell &c.

Remarkable Strata of Flint.

Hutton & Russell Court.

Mercurial Air Pump.

by Sir A. N. Edelcrantz.

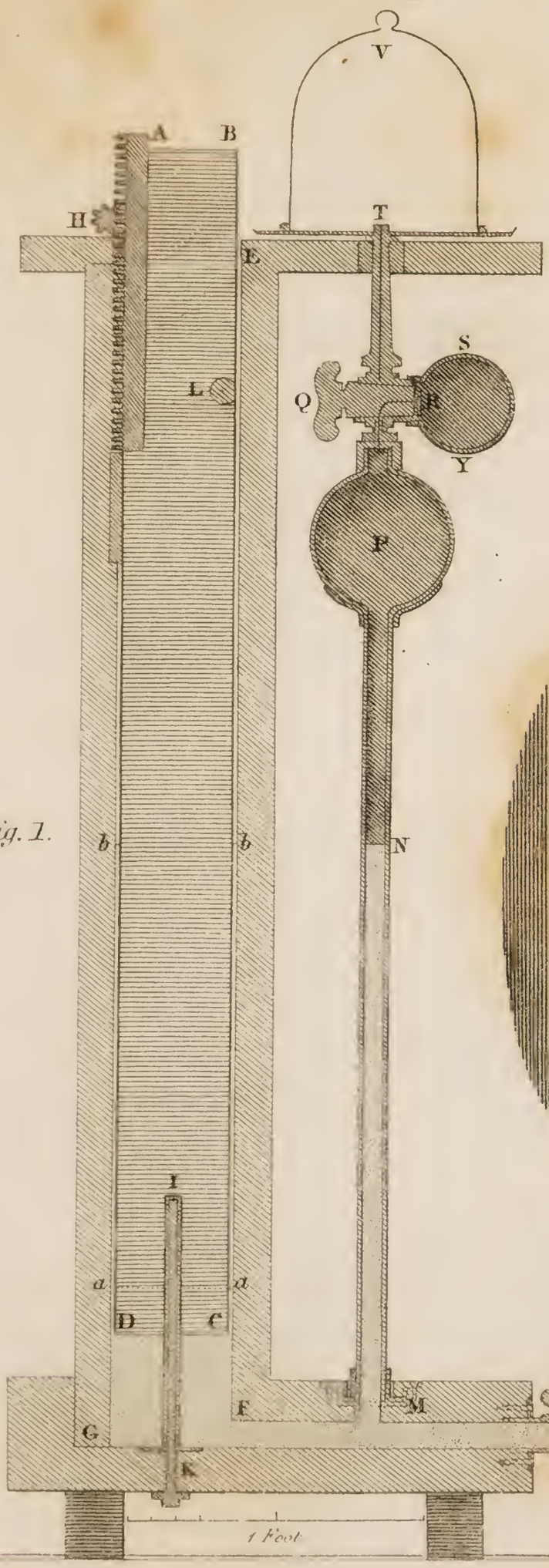
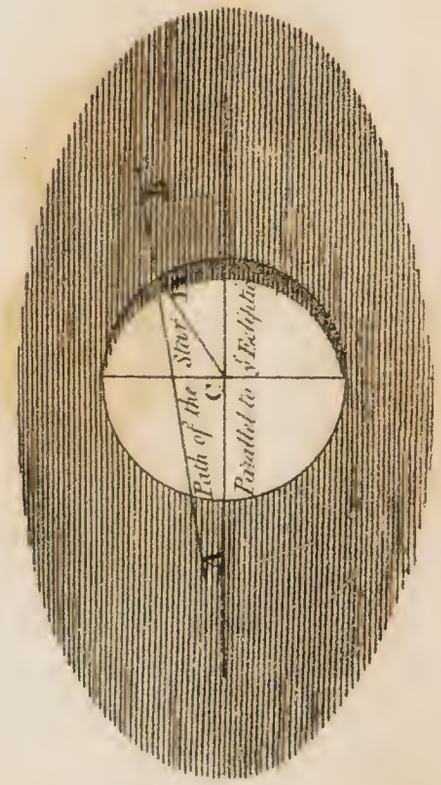


Fig. 1.

Fig. 2.



Mathw Sc. Rayfell Court.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1804.

ARTICLE I.

Experiment proving the Advantage of Periſcopic Spectacles. Communicated in a Letter from W. H. Wollaston, *M. D. F. R. S.*

To Mr. NICHOLSON.

SIR,

THE opinion given by Mr. Jones, in your laſt Journal, reſpecting the improved form of ſpectacle-glaſſes, on which I had delivered my ſentiments in the number preceding (p. 144,) induces me to addreſs you once more upon that ſubject. Mr. Jones's opinion of the improved ſpectacle glaſſes,

It appears wholly unneceſſary to follow Mr. J. through his various obſervations, or to remark upon the experiment by which he deceives himſelf; becauſe all doubt of the advantage of the periſcopic glaſſes may be removed by the following direct comparative trial, which any perſon who chooſes can make without difficulty. referred to experiment.

I have before me two glaſſes, each of four inches "poſitive focus," as propoſed by Mr. J. the one double-convex, which in his judgment is pronounced to be "*indubitably the beſt and moſt convenient that can be deviſed*" (p. 194); the other a concavo-convex, or meniſcus, which he thinks "*evidently the worſt of the two for a ſpectacle-glaſs*," (p. 197.) With the form of lens recommended by Mr. Jones,

When I fix the former at the diſtance for moſt diſtinct viſion oppoſite to a printed octavo page, and approach my eye to the glaſs, the extent of diſtinct viſion compared with that by the

periſcopic glaſs was as 24 to 40,

glaſs, I cannot without pain read quite twenty-four lines; but upon ſubſtituting the periſcopic glaſs fixed in the ſame poſition, I can with *eaſe* diſcern every word in the page, which contains forty lines.

—or rather as 1 to 3.

The enlargement of the field of view obſervable in this trial is ſufficient to evince the ſuperior utility of the periſcopic glaſs; but were there occaſion to compare more nearly the circular areas that may be ſeen with equal diſtinctneſs, they would be found to differ by a ratio as great as that of three to one.

A correſponding advantage in ſhallower lenſes.

This difference is of courſe more evident in glaſſes of ſo high power than in thoſe generally uſed by long-ſighted perſons for common purpoſes; but it cannot be doubted, that a correſponding, though ſmaller, inequality ſubſiſts wherever there is the ſame diſſimilarity of conſtruction, even when the focal diſtance is longeſt.

Concluſion.

The advantage in queſtion is therefore indiſputably proved by direct experiment; to the *novelty* of it Mr. J. himſelf has unintentionally contributed very ſatisfactory evidence; but as to its importance, thoſe alone, who have the misfortune to labour under any defect of viſion, muſt ultimately decide.

I remain, Sir,

Your obliged humble ſervant,

March 20, 1804. W. H. WOLLASTON.

II.

Additional Obſervations on ſome remarkable Strata of Flint in the Iſle of Wight, in a Letter from Sir CHARLES ENGLEFIELD, *Bart. F. R. S. to* JOHN LATHAM, *M. D. F. R. S. and L. S.* (See Page* 183 *of the preſent Number.)*

DEAR SIR,

Obſervations on remarkable ſtrata of flint in the Iſle of Wight.

I FEEL much flattered at the notice taken of my Paper on the chalk pits in the Iſle of Wight by the Linnean Society; and as I wiſh to render my account of the very curious appearances obſerved by me in them as perfect as I can, the following additional obſervations on the ſubject made during a ſecond viſit laſt year are, by your favour, ſubmitted to the Society.

* Linnean Tranſactions, Vol. VI.

The

The pits I laſt year inſpected are as follow, beginning from the eaſt: Obſervations on remarkable ſtrata of flint in the Iſle of Wight.

Brading pit, which is at the eaſtern point of the great ridge, where the valley of Brading-haven interſects it, and ſeparates it from the Yaverland-hill, which terminates in the ſea at Culver and Bembridge. A road cut into the chalk above Knighton. Aſhley-down pit, about three miles eaſt of Newport. A pit very near to and ſouth of Cariſbrook caſtle. The cliffs and caves of Freſhwater bay, both eaſt and weſt of the valley which interſects them entirely and runs from Freſhwater to Yarmouth.

The Yaverland chalk is therefore the only part which I have not examined; and little doubt can be entertained of its ſimilarity to the reſt of the range, to which it evidently belongs.

In Brading pits ſome flints appear in detached nodules, and theſe are found and unbroken.

The inclined ſtrata of flint are viſible, but not to advantage, owing to the manner of working the pits. In theſe ſtrata the flints are univerſally ſhattered, ſome into abſolute powder, others into groſſer powder and fragments mixed. But beſides theſe ſtrata, the chalk in this pit is divided by vaſt perpendicular fiſſures, as ſmooth as plaiſter walls, and in ſome of theſe fiſſures flint has formed, which appears broken like that in the ſtrata.

The road above Knighton only juſt cuts into the chalk ſtratum, but all the flints viſible in the banks are extremely ſhattered.

The pit at the weſt end of Aſhley-down, near two large barrows, is the moſt extenſive and ſatisfactory of any I have ſeen. The perpendicular face of the chalk, where worked, is not leſs than fifty or ſixty feet, and its direction is at right angles to that of the ſtrata, and parallel to their line of dip:—of courſe they are ſeen to very great advantage. The ſtrata ſeems to dip northward more rapidly than in any other place where I could obſerve them. The angle of inclination is from 75 to 80 degrees. There are not layers of flints between every layer of the chalk. Some of the chalk is peculiarly ſolid, and riſes in very large maſſes affecting a cubic form. Their ſolid vein is from twenty-five to thirty feet thick, and is in ſtrata from three to four feet. In all this ſolid part there are very few flints.

Both above and below this harder bed (ſpeaking of the original poſition of the ſtrata) the chalk is ſofter, and has more

Obſervations on remarkable ſtrata of flint in the Iſle of Wight.

flints in it. The ſtratified flints in this pit are full as much ſhattered as any I had ſeen. The nodules are not at all broken. Many of the ſtratified flints are much defaced in this pit by an admixture of pyrites, ſo as to be quite opaque like a coarſe jaſper, and theſe flints are much ſofter than the others, as is always the caſe in the impure flints.

In the chalk pits near Cariſbrook the ſtrata are not ſo viſible as in the pits north of the caſtle (deſcribed in the firſt papers) but the flints are to the full as finely, though perhaps not ſo generally broken. In one flint I obſerved that though it lay in its bed undiſturbed, chalk as if in a fluid ſtate had run into one of the fiſſures. Every appearance of this pit indicates that the chalk, ſince its ſtratification, has received a moſt violent ſhock.

The chalk at Freſhwater bay appears in high perpendicular cliffs, particularly on the weſtern ſide of the bay. Both on the eaſt and weſt the ſtrata dip northward near 80 degrees, and the dip ſeems to run eaſt and weſt very regularly. The weſtern cliff has a very regular and perpendicular face to the eaſtward; and here the parallel direction of the ſtrata, each ſeparated by a thin line of black flint preſents a moſt curious appearance. The flint here is often found in thin plates of conſiderable extent, ſometimes not above an inch thick, and ſeems formed from each ſide of the ſpace which it fills, as the exterior parts (or thoſe neareſt the chalk) are the pureſt and blackeſt, and it is gradually whiter towards the middle, where there is often a ſoft line of chalk included between the two plates of flint. All the ſtratified flints are more or leſs ſhattered, and ſome are reduced to very fine powder. The cave at Freſhwater, which is really a beautiful as well as a very curious one, is formed by the action of the ſea on theſe nearly vertical ſtrata. They are of different hardneſs, and all interſected with fiſſures at right angles to the ſtrata. When the ſea acts on and wears away a ſoft ſtratum, a gallery is formed, and the upper parts of the ſtratum between fiſſure and fiſſure drop out, much in the ſame way as bricks are apt to do out of the flat arch over a window, the harder contiguous ſtrata ſerve as walls to the gallery, but are by degrees perforated in different parts, and become irregular pillars, ſupporting the vaſt weight of the hills above, until the action of the ſea weakens them ſo far that they fall, and a part of the face of the hill goes with them, ſo that the cave is conſtantly, although ſlowly, changing its form.

Larger maſſes of the harder ſtrata, defended by their flint coating, alſo ſtand up in the bay as inſulated rocks of different ſhapes, and much reſembling the Needle Rocks, which are exactly of the materials and formed by the ſame proceſs. In this part of the chalk ſtratum, I ſaw ſeveral foſſils remain which I had ſought in vain in the pits I had viſited. One was ſingular, it had the appearance of part of a very large ſhell, regularly ſtriated, and almoſt flat. I have often ſeen ſmall fragments apparently of a ſimilar ſhell in chalk, but never a large piece. It was ſo firmly fixed as not to be removed without a chiſſel, which I had not.

Obſervations on remarkable ſtrata of flint in the Iſle of Wight.

To theſe obſervations on the chalk of the iſland, I muſt add, that this whole range, although really chalk, is much harder than the chalk of the South-downs, inſomuch that the carpenters cannot uſe it for drawing lines, but import chalk for that purpoſe from Portſdown hill above Portſmouth. They alſo call the iſland chalk by the name of marle, which is, however only the Britiſh name for chalk, and appears in many compound names, ſuch as Marl-borough on the Wiltſhire chalk hills, and the very ſignificant one of Albemarle, or white chalk.

As I have made ſome further obſervations on the ſouthern range of hills which form the back of the iſland, I will treſpaſs on your patience a little longer, particularly as they in ſome degree contradict, or rather correct, what I had advanced on that ſubject in my former letter.

When the northern front of thoſe hills is viewed from Aſhley down, the ſtratum of ſtone mentioned in my former letter as lying directly under the chalk of St. Catharine's and Dunnoſe hills, appears every where to maintain an horizontal poſition; and ſo in its general poſition, particularly in its northern front, it certainly does, but juſt behind the village of Ventnor, the ſtratum entirely diſappears, as if it had been ingulphed in a great chaſm, and a deep and narrow valley runs winding into the chalk hill of St. Boniface, though it does not penetrate through it, which ſeems the remains of the fiſſure into which the ſtone had ſunk.

The appearances of the great ſtone ſtratum, from Niton eaſtward to Ventnor are noted as follows in the journal made on the ſpot.

On an attentive inſpection of the ſtrata of the under cliff, it appears that the great ſtratum of rugged and laminated ſtone, which

Obſervations on remarkable ſtrata of flint in the Iſle of Wight.

which firſt appears at the weſt ſide of St. Catharine's, and thence ranging eaſtward, forms the front of the cliff, overhanging the Underway dips in its ſouthern face gently to the eaſtward. The cliffs at Mirables are much higher above the ſea than thoſe at St. Laurence, and from thence they decline, till at the opening of the hill above Ventnor they totally diſappear. A ſmall crag juſt peeps out of the eaſtern face of this dell, and the whole hill of St. Boniface is, as far as can be ſeen, compoſed of chalk. As however this, like all other chalk hills, is in the ſtate of a ſteep ſlope, covered with turf, perhaps by giving into its face the ſtony ſtratum might be diſcovered. It is alſo to be obſerved that the chalk, which is not viſible above the rock at Mirables, begins to appear ſoon after, and grows gradually thicker as it proceeds eaſtward. At St. Laurence it forms a thick cap to the rocks; and at Steephill Shute, its thickneſs is very much encreaſed, and ſoon after nothing but chalk appears in St. Boniface's hill. In what form the rock re-appears at Dunnoſe to the eaſt of St. Boniface, I have had no opportunity of examining.

I ſhould not, dear Sir, trouble you with theſe defective obſervations, but that every notice, however imperfect, may be of uſe when connected by future obſervations, and that they may ſerve as a ſtimulus to other travellers, who often go over this beautiful line of country, to turn their attention to its ſingular natural phenomena.

I ſend you two ſpecimens of the broken flints, one from Brading, the other from near Cariſbrook, but the tickets are miſlaid, and I am not ſure which is which,

I remain, &c.

Tilney-Street, May 26, 1801.

III.

Account of a Machine for ſweeping Chimnies, by a Blaſt of Air. In a Letter from Mr. J. C. HORNBLOWER, *Engineer.*

To Mr. NICHOLSON,

DEAR SIR,

Introduction.

I SHALL be much obliged if you will allow me a page or two in your Journal for a deſcription of a machine I have lately got up for the purpoſe of cleanſing chimnies from ſoot. My

wiſh is, if poſſible, to keep alive the ſpark that has been kindled by a truly philanthropic individual, with a view to reſtore a degraded claſs of our ſpecies to their rank in civil ſociety. The machine, as it is conſtructed ſolely for the purpoſe mentioned above, is certainly novel in its intention, and without a better acquaintance with its principle and operation than the public are at preſent in poſſeſſion of, it is not likely to gain that notice which in my judgment it is entitled to. There is a propenſity in us to turn into ridicule any thing that will admit of a ridiculous conſtruction, or afford an opportunity for the diſplay of our wit; and ſuch is the unfortunate circumſtance of this machine, that grinning takes precedence of gravity the moment it is announced as a candidate for the gold medal at the Adelphi. However, happy for it, and thrice happy for me, there is a tribunal which can judge fairly and ſcientifically of its merits, for which I preſent it as follows.

Propenſity to ridicule inventions.

The body of the machine A, *Plate* XIII. is made of copper, of about 3lb. to the ſquare foot, and its contents are about three cylindrical feet. In the middle of the cover, which is ſoldered on, is a ſyringe or condenſer, having its handle as appears above the cover. On one ſide of the cover is inſerted a crooked pipe, having a valve opening inward in its inner or lower end; the ſtem of which comes up through the pipe, and terminates with a button C. The pipe is continued by a flexible leather one, to which is united the tin plate tube B B.

Deſcription of the apparatus. A veſſel, into which air is condenſed, communicates with a tube charged with ſmall gravel, which being blown up the chimney brings down the ſoot.

This tube B B has a croſs bar of tin plate fixed edge-ways in the bottom or breech of the tube which detaches from the leather pipe ſomewhat like a piſtol barrel, leaving a piece of tube about ſix inches remaining to the leather pipe, into the bottom of which this caſe bar is fixed, and ſerves to receive a charge of ſmall gravel, having a piece of paper firſt laid on the croſs bar; the other part of the tube is then to be replaced, and the air-veſſel ſuppoſed to be full; the valve is preſſed down by a little lever accompanying the machine, and its contents are diſcharged into the ſhaft of the chimney; and if there is any ſoot worth ſweeping away, it will come down.

Obſervations concerning the Machine.

I do not pretend to have made any new diſcovery in the conſtruction or application of the machine, for it is well known that a muſket has often been applied for the ſame purpoſe, and with

This operation is ſimilar to firing a muſket up the chimney.

with as good effect as need be, barring the hazard of ſetting fire to the ſoot; and indeed, when a chimney has accidentally taken fire, it is the beſt and moſt expeditious mode of putting it out, at leaſt of bringing the ſoot into a ſituation by which it may be extinguiſhed.

Eſtimate of the velocity of the extended air, and its effect in the chimney; together with that of the gravel.

I have ſaid that the veſſel contains nearly three cylindrical feet, and we cannot crowd three atmoſpheres of air into it, in which caſe there will be 42 lbs. per inch ſquare at round numbers, preſſing againſt the charge in the tube, or rather againſt the valve, the tube being $2\frac{1}{4}$ inch diameter, which amounts to 168 lbs. for the whole area. This air will all be diſcharged in one ſecond, the mean velocity of which may be fairly reckoned at 50 feet, in that time having impetus in itſelf ſufficient to carry away any ſoot of conſequence in a chimney of 100 feet high; but when we take into the account the charge of gravel, being alternately incident and reflected on all ſides of the chimney, we need not fear to affirm that it is perfectly applicable to its intention.

Additional tubes for curved chimnies.

To render it as univerſal as poſſible, there muſt be another tube, to be occaſionally uſed, when the ſide of the chimney near the fire place is gathered over in order to bring the throat of the flue over the fire, as at *Fig.* II. from *a* to *b*, or it ſhould be in ſeveral pieces, to conform to the height of this gathering, and in this caſe the charge muſt be at the upper fiſſure at *b*, and the pipe ſtayed as perpendicularly as can be conveniently done; for which purpoſe there is a piece made to fix on the pipe, having two ſtems, as *Fig.* III. which if put in the bearing ſide of the pipe, will keep it upright and in the crater of the flue. The machine thus conſtructed is to be laid on a little truck, and tranſported from houſe to houſe, with a gallon of gravel to begin with; becauſe until it has obtained the good opinion of the public, it would not be neceſſary to attempt ſuch regulations as would afterwards be deemed requiſite to facilitate the operation.

Trial at the houſe of the Society of Arts in the Adelphi

This poor thing, brought forth under other diſadvantages beſides its ſingularity, was conducted to the Adelphi, to undergo an eventful trial, in competition with ſome others, who had no ſuch ſingularity to be imputed to them; they being all deſcendents of the ſtock of one family, the remoteſt relation between each not being more diſtant than couſin-german.—

Other machines exhibited there.

One of them however poſſeſſed great originality and ingenuity in

in the deſign. It conſiſted of a congeries of bruſhes, which in preparation for action were collapſed by their conſtruction ſo as to admit of being thruſt up the chimney with very little reſiſtance, and when it had gained the top, on pulling a ſtring, it expanded to the dimenſions of the chimney, and brought the ſoot down before it; but from want of a mechanical attention to its conſtruction, ſome parts being too ſtrong, and ſome too ſlight, it broke in the firſt experiment. Bruſhes.

Another (very elegant inſtrument) was preſented to the committee, conſiſting of a large bruſh, ſuch as I have ſeen in ſome churches to ſweep down the cobwebs, with a ſcraper attached to it; lengthened to the height of the chimney by a ſeries of whalebone rods, whipped together as in the conſtruction of waggoners' whips, very accurately jointed to each other. Indeed this was eſſential and common to them all, except one; and that was a bruſh, not to be ſent up the chimney, but down it, by getting on the top of the houſe, and putting it into the chimney, and ſo letting it fall to the bottom. However, it was ſaid, they would all do very well but mine, as a machine like that was adopted to a rectilinear flue only; whereas theirs had ſo many joints and other properties to conform to a curvature, that they would be ſure to go through a crooked flue as well as a ſtrait one. A ſingle bruſh. Notion that the author's machine is ſuited only to a ſtrait flue.

When the late Doctor Johnſon had any thing advanced to him of the marvellous kind, his way of heſitation to admit it, was by ſaying "It may be very true, but it is impoſſible."—So I ſay with reſpect to the diſpoſition of theſe rods, when ſet in competition with a volume of air going at the rate of fifty feet in a ſecond from the lower part of the chimney to the top. What is there to ſtop it? What is there to prevent its conformity to the curvature or *angles*, if you will? The ſame denied.

The manner in which theſe bruſhes act, let their conſtruction be what it may, is by ſtriking off the ſoot. What does a boy within the flue with the bruſh in his hand? To be ſure ſweeping is the moſt appropriate term for what he does; but then, unleſs, we attend to the peculiarity of the action, and conſider how far it is poſſible to effect that ſweeping by another mode of action, we ſhall not get any further than the boy in the flue. But is it not as eaſy to conceive of a quantity of air being put into action along a road or a ſtreet, ſo as to ſweep away the duſt before it? No; you will ſay, it blows it along; To *blow* down the ſoot is as natural as to *ſweep* it down.

but

but the term blowing aſſociates only with the accuſtomed notion of the action of wind, and was the effect to be produced by the agency of ſteam; the term blowing would more eaſily be diſmiſſed, and we might call the action *driving* it along.

The term ſweeping is in ſome inſtances uſed to expreſs the action of wind.

When we aſſociate this agency of the wind with its effect on a field of ripening corn, we conceive of it *ſweeping* along or over the corn; and the ſame effect would follow, were we to conceive of a large bruſh of long hair carried on the tops of corn; ſo that however popular expreſſion, or the prejudice of cuſtom may operate to diſtinguiſh the cauſes producing the ſame effect, it becomes us to waive ſuch diſtinctions, and admit the efficiency of the one as much as the other.

Inſtances of the action of wind in a chimney.

I have obſerved that when a door has been ſuddenly opened or ſhut, it has brought down ſoot in a chimney which has a ſluggiſh draft; or a piece of paper accidentally thrown on the fire has had the like effect; and this by a momentary acceleration of the current of air in the flue. What then will not ſuch a machine as this perform, with ſuch a volume of air, with ſuch a velocity.

The air machine ſweeps chimnies of all heights, and is durable.

One circumſtance preſents itſelf, by which the air machine muſt have a decided preference over the bruſhes; which is this, you muſt know the height of the chimney, and adapt the length of the rods to that height, or elſe you will not know when you are in the flue or out of it. Whereas my machine knows no neceſſity for ſuch a punctilio; all that is neceſſary being only to give ſo many ſhocks of the condenſer to a chimney of two ſtories, and ſo many to one of three, and ſo on. And again, theſe bruſhes in the very outſet of trial made with them, are ſubject to accidents, and will moſt aſſuredly wear out very faſt. What muſt become of thoſe whalebone rods when the ſewing is rubbed through in paſſing up and down againſt the projections of unceremonious bricks and mortar? What repairs will they not be ſubject to in the courſe of one day's action? Whereas the air machine will ſweep a hundred chimnies, and be repaired for two-pence; it wanting only a little oil in the condenſer.

It was tried at the Adelphi upon chimneys that contained no ſoot.

When this machine was carried to the Adelphi, it came to be the third in courſe for the trial, and it was ordered *down*, *down* to the foundation of the houſe, for fear a chimney ſhould not be found high enough in any other part of the building.—But when it was brought, behold there had been two ſweepers already, which obliged me to apologize for the machine, by obſerving

obſerving that they had taken down all the ſoot, and left nothing for it to do *. Upon which a ſuggeſtion was made, that there was another chimney which had a fire alternately during the winter †; and on examination it was agreed, that, by the appearance of the ſoot about the breaſt of the flue, the chimney was very foul, of which, however, I had my doubts. But it was thought expedient to try that chimney; and the firſt charge we gave it brought no ſoot. We gave it another charge, but no ſoot came; and it was thought beſt to ſend up a boy, with proper implements, to ſweep the chimney; but no ſoot came down, except what may be ſaid to be brought away by violence, and did not exceed a quart-potful, and with it much mortar or pargetting.

This brings me to ſome obſervations I have made on the condition of chimnies in general, as to the ſoot they may contain.

Trials of the machine with other chimnies.

I tried this machine at Shoreditch Workhouſe, where the chimnies are very lofty, for which reaſon I preferred them; but when I came to ſee their circumſtances I ſuſpected how it was, and we tried two of them where the fires had been very economical, and may continue ſo winter after winter, without ever requiring to be ſwept at all, for reaſons I will give by and by. I then went to a neighbour of mine, and we ſwept two of his chimnies; one had no ſoot, the other had a good deal; and we thought fit to get a boy to go up the laſt chimney, and he brought down about a hatful of ſoot; however, my neighbour was ſatisfied, and ſigned my certificate to the Committee at the Adelphi.

We then went to another neighbour's, and ſwept his parlour chimney, and diſlodged a great quantity of ſoot. We were to have ſwept his kitchen chimney, but it having a ſmoke-jack, we could not apply the machine for want of the additional pipe in *Fig.* 2.

* It appears the committee had no regard to the effect of any of the candidate's inventions, any further than a fair thruſt up the chimney.

† With another in the ſame room I ſuppoſe.

Account

IV.

Account of the Changes that have happened, during the laſt Twenty-five Years, in the relative Situation of Double Stars; with an Inveſtigation of the Cauſe to which they are owing. By WILLIAM HERSCHEL, *LL. D. F. R. S. From the Philoſophical Tranſactions for* 1803.

(Concluded from Page 227.)

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

I SHALL now enter into a more detailed examination of the ſeveral angles of poſition I have taken at different times, and ſhow that they agree perfectly well with the appearances which muſt ariſe from the revolution of a ſmall ſtar round Caſtor. A calculation of theſe angles may be had, by finding the annual motion of the ſmall ſtar, from the change of 21° 54′, which has been ſhown to have taken place in 23 years and 142 days. Accordingly, I have given, in the firſt column of the following table, the time when the angles were taken. In the ſecond, are the angles as they were found by meaſure; they are all in the north-preceding quadrant. The third column contains a calculation from the annual motion of 56′,18, obtained as before mentioned: it ſhows what theſe angles ſhould have been, according to our preſent ſuppoſition of a revolving ſtar. And the laſt column gives the difference between the obſerved and calculated angles.

Times of the obſervations.	Obſerved angles.	Calculated angles.	Differences.
Nov. 5, 1779 -	32° 47′	32° 47′	0° 0′
Feb. 23, 1791 -	22 57	22 11	+ 0 46
Feb. 26, 1792 -	27 16	21 16	+ 6 0
Dec. 15, 1795 -	13 52	17 42	− 3 50
March 26, 1800	18 8	13 41	+ 4 27
April 23, 1800	10 30	13 37	− 3 7
Dec. 31, 1801 -	7 58	12 2	− 4 4
Jan. 10, 1802 -	10 53	12 1	− 1 8
Jan. 23, 1802 -	10 28	11 59	− 1 31
Feb. 28, 1802 -	13 0	11 53	+ 1 7
Feb. 11, 1803 -	7 53	11 0	− 3 7
March 23, 1803	13 23	10 54	+ 2 29
March 27, 1803	10 53	10 53	0 0

On

On looking over the fourth column of this table, it will be found, that the differences between the obſerved and calculated angles are not greater than may be expected, conſidering that moſt of the early meaſures are ſingle, and cannot have the accuracy which may be obtained by repetition. Even as they are, we muſt acknowledge them ſufficient to aſcertain the gradual change in the angle of poſition of the two ſtars. In one place, the difference amounts to ſix degrees; but it will ſoon appear, that a more accurate annual motion gives a calculated poſition which takes off much of the error of this meaſure.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

In a converſation with my highly eſteemed friend the Aſtronomer Royal, he happened ſome time ago accidentally to mention, that Dr. Bradley had formerly obſerved the two ſtars of α Geminorum to ſtand in the ſame direction with Caſtor and Pollux. It occurred to me immediately, that if the time of this obſervation could be nearly aſcertained, it would be of the greateſt importance to the ſubject at preſent under conſideration. For, ſhould Dr. Bradley's poſition be very different from a calculated one, it would induce us at once to give up the idea of a revolving ſtar. The obſervation was made by Dr. Bradley with a view to ſee whether any change could be perceived in the courſe of the year, by which the annual parallax of the ſtars might be diſcovered. Dr. Maſkelyne, who had this information from Dr. Bradley in converſation, had made a memorandum of it in his papers. He has been ſo kind as to look for it; and, as ſoon as he found the note, he ſent me the following copy, which I have his permiſſion to tranſcribe.

"*Double ſtar Caſtor. No change of poſition in the two ſtars: "the line joining them, at all times of the year, parallel to the line "joining Caſtor and Pollux in the heavens, ſeen by the naked "eye.*"

Dr. Maſkelyne informs me, that the obſervation muſt have been made about the year 1759; and alſo mentions, that he himſelf verified the fact, as to the line joining the two ſtars appearing through the teleſcope parallel to the line joining Caſtor and Pollux, in 1760 or 1761; but that he did not examine it at various times of the year.

The advantage of having an angle of poſition obſerved in 1759 by Dr. Bradley, and ſo ſoon after verified by Dr. Maſkelyne, will give us an addition of 20 years to our period. On calculating the right aſcenſion and polar diſtance of Caſtor and

Pollux

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

Pollux for November 5, 1759, it appears, that a line drawn from Pollux through Caſtor, muſt have made an angle of 56° 32′ north preceding with the parallel of that ſtar; and, this being alſo the poſition of our double ſtar, we have an interval of 43 years and 142 days, for a change of 45° 39′, from the time of Dr. Bradley's obſervation to that of my laſt meaſure of the angle. By this we are now enabled to correct our former calculation, which was founded upon a ſuppoſition that the firſt angle of poſition I had taken was perfect; but this could hardly be expected, and on examination it appears that the meaſure was 2° 40′ too little. The annual motion, by our increaſed period, is 1° 3′,1; and the computation of the angles of poſition in the third column of the following table, as well as the differences contained in the fourth, are made according to this motion.

Times of the obſervations.	Obſerved angles.	Calculated angles.	Differences.
Nov. 5, 1759 -	56° 32′	56° 32′	0° 0′
Nov. 5, 1779 -	32 47	35 29	− 2 42
Feb. 23, 1791 -	22 57	23 36	− 0 39
Feb. 26, 1792 -	27 16	22 32	+ 4 44
Dec. 15, 1795 -	13 52	18 32	− 4 40
March 26, 1800	18 8	14 3	+ 4 5
April 23, 1800	10 30	13 58	− 3 28
Dec. 31, 1801 -	7 58	12 12	− 4 14
Jan. 10, 1802 -	10 53	12 10	− 1 17
Jan. 23, 1802 -	10 28	12 7	− 1 39
Feb. 28, 1802 -	13 0	12 1	+ 0 59
Feb. 11, 1803 -	7 53	11 1	− 3 8
March 23, 1803	13 23	10 54	+ 2 29
March 27, 1803	10 53	10 53	0 0

When the reſult of this table is compared with that of the former, it will be ſeen that my obſervations agree not only very well with Dr. Bradley's poſition, but even give more equally divided differences than before, ſo that the exceſs and differences counteract each other better than in the firſt table.

The time of a periodical revolution may now be calculated from the arch of 45° 39′, which has been deſcribed in 43 years and 142 days. The regularity of the motion gives us great reaſon to conclude, that the orbit in which the ſmall ſtar moves about

about Caſtor, or rather, the orbits in which they both move round their common centre of gravity, are nearly circular, and at right angles to the line in which we ſee them. If this ſhould be nearly true, it follows, that the time of a whole apparent revolution of the ſmall ſtar round Caſtor, will be about 342 years and two months.

Obſervations and inferences reſpecting the change of relative ſituation in ſtars extremely near each other.

γ Leonis.

Our foregoing diſcuſſions will greatly abridge the arguments which may be uſed, to ſhew that this ſtar and its ſmall companion are alſo probably united in forming a binary ſyſtem. But, in order to give more clearneſs to our diſquiſition, we ſhall follow the arrangement which has been uſed with α Geminorum, and prefix the ſame letters to our paragraphs. Then, if any one article ſhould appear to be not ſufficiently explained, we need but turn back to our firſt double ſtar, where the ſame letter will point out what has already been ſaid more at large on the ſubject; and an application of it may eaſily be made.

The diſtance of the ſtars γ and *x*, as I ſhall again call the ſmall one, has undergone a viſible alteration in the laſt 21 years. The reſult of a great number of obſervations on the vacancy between the two ſtars, made with the magnifying powers of 278, 460, 657, 840, 932, 1504, 2010, 2589, 3168, 4294, 5489, and 6652, is, that with the ſtandard power and aperture of the 7-feet teleſcope, the interval in 1782 was $\frac{1}{4}$ of a diameter of the ſmall ſtar, and is now $\frac{3}{4}$. With the ſame teleſcope, and a power of 2010, it was formerly $\frac{1}{2}$ of a diameter of the ſmall ſtar, and is now full 1 diameter. In the years 1795, 1796, and 1798, the interval was found to have gradually increaſed; and all obſervations conſpire to prove, that the ſtars are now $\frac{1}{2}$ a diameter of the ſmall one farther aſunder than they were formerly. The proportion of the diameter of γ to that of *x*, I have, by many obſervations, eſtimated as 5 to 4.

The firſt meaſured angle in 1782, is 7° 37′ north following *; and the laſt, which has been lately taken, is 6° 21′ ſouth following.

* In my ſecond Catalogue of double Stars, (Phil. Tranſ. for 1785, page 48,) the angle of poſition is 5° 24′. This was taken April 18, 1783; and, not being acquainted with the motion of the ſmall ſtar, I ſuppoſed it to be more accurate than the former meaſure.

Obſervations and inferences reſpecting the change of relative ſituation in ſtars extremely near each other.

lowing. The ſum of theſe angles gives 13° 58′, for the change that has taken place in 21 years and 38 days. To account for this, we are to have recourſe, as before, to the various motions of the three bodies.

Single Motions.

(*a*) The motion of *x* alone cannot be admitted, ſince it is known that γ Leonis is not at reſt. The annual proper motion of this ſtar, according to M. De la Lande, is + 0″,38 in right aſcenſion, and 0″,04 in declination towards the ſouth.

(*b*) γ cannot be the only moving body; becauſe its motion in right aſcenſion only, which, in 21,1 years, at the parallel of γ, amounts to 7″,49, would have long ago taken it away from the ſmall ſtar.

(*c, d, e,*) The ſun cannot be the only moving body; becauſe its motion in right aſcenſion will not account for that of γ Leonis, which ſtar therefore cannot be at reſt. And, if we were willing to give up the former aſſumed ſolar motion, in order to fix upon ſuch a one as would explain the motion of γ, we ſhould be under a neceſſity to contradict the united evidence of the proper motions of many principal ſtars which are in oppoſition to it.

Double Motions.

(*f*) When two motions are propoſed, we cannot fix upon γ and *x* for the moving bodies, unleſs we ſhould ſet aſide the ſolar motion, and this, we know, cannot properly admit of a doubt.

(*g*) That we cannot allow O and *x* to be the two bodies in motion, follows from the inſufficiency of the ſolar motion to account for that of γ, which muſt be real, or at leaſt partly ſo.

(*h*) If O and γ are the moving bodies, the given ſituations of *x*, in the years 1782 and 1783, point out an apparent motion of *x*, which muſt be intirely owing to the ſolar parallax; and, therefore, thoſe who will admit this hypotheſis, muſt grant the diſcovery of the motion of the ſolar ſyſtem, and of the proportional parallax of the two ſtars γ and *x*. Let us however examine whether any motion of the ſun, ſuch as we can admit, will account for the change of poſition and diſtance pointed out by my obſervations of the ſmall ſtar near γ Leonis.

The

The joint effect of proper motion and parallax, has carried γ from its ſituation in 1782 to that where we now find it. The ſmall ſtar, having all this time, in appearance, accompanied γ, muſt have gone through a ſpace of 7″,98, in a direction which makes an angle of 8° 30′ ſouth following with the parallel of γ, in order to be at its preſent diſtance from it, and at the ſame time to have undergone the required change of its angle of poſition. Now, as the ſuppoſition we are examining requires this ſmall ſtar to be actually at reſt, it will be neceſſary to aſſign to the ſun an oppoſite motion of the ſame velocity, in order to make that of x only an apparent one. The conſequence of this will be a retrograde motion of the ſun, which it is well known cannot be admitted.

Obſervations and inferences reſpecting the change of relative ſituation in ſtars extremely near each other.

Motion of the three Bodies.

(*i*) A motion of all the three bodies, is the only way left to explain the phenomena of our double ſtar; and I ſhall now again point out the very particular circumſtances which it is requiſite ſhould all happen together, to produce the intended effect.

Let the motion of the ſun, with the ſame annual velocity 1, as in the caſe of α Geminorum, be directed towards λ Herculis. Then the effect of this motion will ſhew itſelf at the place of γ Leonis, in the annual velocity of ,3314, and in a direction which makes an angle of 31° 11′ ſouth preceding with the parallel of that ſtar. In this calculation, I have admitted the diſtance of the largeſt of the two ſtars of γ from the ſun to be 3, that of α Geminorum being 2. But, if any other diſtance ſhould hereafter be conſidered as more probable, the calculation may be eaſily adapted to it. The conſequence of the parallax thus produced on γ Leonis in 21,1 years, will be an apparent motion of 2″,788 ſouth preceding, in the abovementioned direction; and, on x, it will be in the ſame time, and in the ſame direction, 1″,091. As the ſmall ſtar muſt not be too near γ, we have, in the calculation, ſuppoſed it to be at the diſtance of 4 from O.

The real annual proper motion of γ is required to be 3,5202; and its direction muſt make an angle of 3° 40′ north following with the parallel. By this motion alone, γ would have paſſed over a ſpace of 9″87 in 21,1 years; but, when it is combined with the apparent motion ariſing from parallax, the ſtar will come into its preſent ſituation.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

The real annual motion of x muſt be 4,6294, in a direction 0° 20′ ſouth following. This will carry it over 9″,74, in 21,1 years; and, when combined with the apparent motion which the ſolar parallax will occaſion, both together will bring it to its proper diſtance from γ Leonis, and to a ſituation which will agree with the laſt obſerved angle of poſition.

From what has been ſaid, it is again evident, that not only as many particular circumſtances muſt concur in explaining the phenomena of γ Leonis as we have pointed out with α Geminorum, but that a very marked condition is added in our ſecond double ſtar, which requires an adjuſtment of velocities in γ and x, which ſhall alſo fit the ſame ſolar motion that was uſed in α Geminorum. And this proves, that every additional double ſtar which requires the ſame condition in order to have its appearances explained, will inforce the arguments which have been uſed, in a compound ratio.

If, on the other hand, we have recourſe to the ſimplicity of the known effects of attraction, and admit the two ſtars of our preſent double ſtar to be united in one ſyſtem, all the foregoing difficulties of accounting for the obſerved phenomena will vaniſh. Whatever may be the proper motion of the ſun, the parallax ariſing from that cauſe will affect both ſtars equally, on account of their equal diſtance from the ſun. The proper motion of γ Leonis alſo may be in any direction, and of any given velocity, ſuch as will agree beſt with aſtronomical obſervations; ſince the motion of a ſyſtem of bodies will not interfere with the particular motion of the bodies that belong to it, ſo that our ſecondary ſtar will continue its revolution round the primary one without diſturbance.

It will now be neceſſary to examine the obſerved angles of poſition, and to compare them with calculated ones; but, as there has been a change in the diſtance of the two ſtars, it is evident that, if they revolve in circular orbits, the ſituation of the plane of their revolution muſt be conſiderably inclined to the line in which we ſee the principal ſtar.

Let N F S P, Fig. 2, be the orbit in which x revolves about γ placed in the centre. Suppoſe a perpendicular to be erected at γ leading to O, not expreſſed in the figure. By an obſervation of Feb. 16, 1782, we have the angle F γ x = 7° 37′ north following; and the proportion of the apparent diameter of γ to that of x has been given as 5 to 4. It has alſo been aſcertained

tained, that the vacancy between the apparent diameters, when the firſt angle of poſition was taken, was $\frac{1}{4}$ diameter of the ſmall ſtar; and the laſt angle of poſition being 6° 21′ ſouth following, with a diſtance between the ſtars of $\frac{3}{4}$ diameter of the ſmall ſtar, we obtain the two points or centres of the ſmall ſtars $x x'$, through which an ellipſis $a b x x' c d$ may be drawn about γ. This will be the apparent orbit in which the ſmall ſtar will be ſeen to move about γ, by an eye placed at O. And the inclination of the orbit to the line in which we ſee the double ſtar, will be had ſufficiently accurate to enable us to give a calculation of the ſeveral angles of poſition that have been taken. The ellipſis we have delineated ſhews that the ſmall ſtar, in its firſt ſituation x, could not be much paſt its conjunction at b, and that, conſequently, in paſſing from x to x', the parts of the apparent elliptical arch, which are projections of the real circular arch $h h'$, would be deſcribed in times nearly proportional to the time in which the whole arch has been deſcribed. Upon theſe principles, the third column of the following table has been calculated.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other,

Times of the obſervations.	Obſerved angles.	Calculated angles.	Differences.
Feb. 16, 1782 -	7° 37′ *nf*	7° 37′	0° 0′
April 18, 1783 -	5 24 *nf*	6 51	− 1 27
Jan. 24, 1800 -	3 16 *sf*	4 15	− 0 59
Feb. 19, 1800 -	3 23 *sf*	4 18	− 0 55
March 26, 1800	3 47 *sf*	4 22	− 0 35
Jan. 26, 1802 -	6 4 *sf*	5 35	+ 0 29
Feb. 10, 1803 -	3 33 *sf*	6 16	− 2 43
March 22, 1803	6 32 *sf*	6 20	+ 0 12
March 26, 1803	6 21 *sf*	6 21	0 0

The difference between the calculated and obſerved angles, contained in the fourth column of the preceding table, is ſo little, that we may look upon the gradual change of theſe angles as eſtabliſhed by obſervation; and we may form a calculated eſtimate of the time which will be taken up by the mutual revolution of the two ſtars. The apparent places $x x'$, being referred to their real ones, give the arch $h h'$, which has been deſcribed in 21 years and 38 days; and this arch, ſeen from the centre γ, is about 6° 20′: it follows, that the length of a whole revolution of our ſmall ſtar round γ Leonis, will be about 1200 years.

ε Bootis.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

This beautiful double ſtar, on account of the different colours of the ſtars of which it is compoſed, has much the appearance of a planet and its ſatellite, both ſhining with innate but differently coloured light.

There has been a very gradual change in the diſtance of the two ſtars; and the reſult of more than 120 obſervations, with different powers, is, that with the ſtandard magnifier, 460, and the aperture of 6,3 inches, the vacancy between the two ſtars, in the year 1781, was $1\frac{1}{2}$ diameter of the large ſtar, and that it now is $1\frac{3}{4}$. By ſome earlier obſervations, the vacancy was found to be conſiderably leſs in 1779 and 1780; but the 7-feet mirror then in uſe was not ſo perfect as it ſhould have been, for the purpoſe of ſuch delicate obſervations. By many eſtimations of the apparent ſize of the ſtars, I have fixed the proportion of the diameter of ε to that of *x*, as 3 to 2. Auguſt 31, 1780, the firſt angle of poſition meaſured 32° 19′ north preceding *; and, March 16, 1803, I found it 44° 52′, alſo north preceding: the motion, therefore, in 22 years and 207 days, is 12° 33′. It ſhould alſo be noticed, that while the apparent motion of α Geminorum, and of γ Leonis, is retrograde, that of ε Bootis is direct.

A proper motion in this ſtar, if it has any, is ſtill unknown; our former arguments, therefore, cannot be applied to it, without ſome additional conſiderations; and, as many others of my double ſtars will ſtand in the ſame predicament, I ſhall give an outline of what may be ſaid, to ſhow that this, and probably many of the reſt, are alſo binary ſyſtems.

Single Motions.

(*a*—*e*) If ε Bootis is a ſtar in which no proper motion can be perceived, we may infer, from the highly probable motion of the ſolar ſyſtem, that this ſtar, which is of the third magnitude, and on that account within the reach of parallax, muſt have a real motion, to keep up with the ſun, in order to prevent an

* The angle of poſition, in my firſt Catalogue of double Stars, Phil. Tranſ. for 1782, page 115, is 31° 34′ (it ſhould be 54′) north preceding. This will be found to be a mean of the three firſt meaſures hereafter given in a table of poſitions.

apparent

apparent change of place, which muſt otherwiſe have happened. In this caſe, no ſingle motion can be admitted to explain the phenomena of our double ſtar. But, if a real proper motion of ε Bootis ſhould hereafter be aſcertained, the arguments we have uſed in the caſe of γ Leonis, will lead to the ſame concluſion. Obſervations and inferences reſpecting the change of relative ſituation in ſtars extremely near each other.

Double Motions.

(*f*) ε and *x* cannot be the moving bodies; and our former argument (*f*) will apply to every double ſtar whatſoever.

(*g*) O and *x* cannot be alone in motion; for, if no motion in ε can be perceived, it muſt move in a ſimilar manner with the ſun, and none of the three bodies will be at reſt. But, if its proper motion ſhall hereafter be found out, it muſt either be exactly the reverſe of the ſolar motion, and therefore only an apparent one, or it will be more or leſs different. In the latter caſe, all the three bodies muſt be in motion; in the former, the exact quantity of the ſolar motion will be diſcovered, and the relative parallax of many ſtars may be had by obſervation.

(*h*) If O and ε are the two bodies in motion, and if at the ſame time no motion in ε can be perceived, then the apparent motion of *x* muſt be intirely owing to the different effect of the ſolar parallax on ε and *x*; but the effect of the ſolar parallax on *x*, can only be in a direction contrary to the motion of the ſun, which, being north following the ſmall ſtar, whether it be nearer or farther from us than ε, muſt have an apparent motion towards the ſouth preceding part of the heavens. But this is directly in oppoſition to my obſervation of the motion of the ſmall ſtar, which, theſe laſt 23 years, has been directed towards the north following.

Motion of the three Bodies.

(*i*) Let the motion of the ſun be again towards λ Herculis; then, if no motion in ε Bootis be perceivable, it muſt move exactly like O. Highly improbable as it is, let it be admitted.

Then, in addition to this extraordinary ſuppoſition, a third motion is alſo required for *x*, which, aided by the ſolar parallax, is to carry it likewiſe within a quarter of a diameter of ε, into the ſame place where, though unperceived, the large ſtar has been carried by its own motion; that is, in order to be apparently

Obſervations and inferences reſpecting the change of relative ſituation in ſtars extremely near each other.

rently at reſt, the ſun, ε Bootis, and its ſmall companion, muſt all move exactly alike, ſetting aſide the very little difference in the poſition and diſtance of the ſmall ſtar, which, in the whole, amounts to little more than 6-tenths of a ſecond; than which, certainly nothing can be more improbable.

But, if ε ſhall hereafter be found not to have been at reſt during the time of my obſervations upon it, then its place will be given; and, ſince alſo the ſituation of x, with reſpect to ε, is to be had from my angles of poſition and diſtances of the two ſtars, the caſe will be ſimilar to that which has already been conſidered, in the paragraph (*i*), under the head of γ Leonis.

I may here add a remark with regard to ε Bootis, which will be applicable to ſeveral more of my double ſtars. In the milky-way, a multitude of ſmall ſtars are profuſely ſcattered, and their arrangement is very different from what we perceive in thoſe parts of the heavens which are at a conſiderable diſtance from it. About ε Bootis, which is ſituated in what I have formerly called figuratively a nebulous part of the heavens *, there are, comparatively ſpeaking, hardly any ſtars; and, that ſo remarkable a ſtar as ε ſhould have a companion, ſeems almoſt to amount to a proof that this very companion is, as it appears to be, a connected ſtar. The *onus probandi*, therefore, ought in juſtice to fall to the ſhare of thoſe who would deny the truth of what we may call a fact; and I believe the utmoſt they could do, would be to prove that we may be deceived; but they cannot ſhow that this ſtar has no connection with ε Bootis.

This argument will be much ſupported, when we conſider that many of the double ſtars in the milky-way are probably ſuch as have one of the ſcattered ſtars, nearly in the ſame line, at a great diſtance behind them. In this caſe, the two ſtars of the double ſtar have no connection with each other; and the great number of them in the milky-way, is itſelf an indication of this effect of the ſcattered multitude of ſmall ſtars. In the ſingle conſtellation of Orion, for inſtance, we have no leſs than 43, pointed out by my catalogues; ten of which are of the firſt claſs, and yet have undergone no change of diſtance or poſition ſince I firſt perceived them. But, with apparently inſulated ſtars, ſuch as ε Bootis, the caſe is juſt the reverſe.

* See Phil. Tranſ. for 1784, page 449.

If,

If, in conſequence of our former arguments, and the preſent remarks, we place ε Bootis among the ſtars which hold a ſmaller one in combination, we may delineate its orbit as in Plate VIII. Fig. 3.

Obſervations and inferences reſpecting the change of relative ſituation in ſtars extremely near each other.

Let P N F S repreſent a circle, projected into the elliptical orbit *a x x′ b c d*. ε is the large ſtar; and *x x′* are the firſt and laſt meaſured north preceding ſituations of the ſmall one, as given in the following table.

Times of the obſervations.	Obſerved angles.	Calculated angles.	Differences.
Auguſt 31, 1780	32° 19′	33° 58′	− 1° 39′
March 13, 1781	30 21	34 13	− 3 52
May 10, 1781 -	33 1	34 18	− 1 17
Feb. 17, 1782 -	38 26	34 40	+ 3 46
Auguſt 18, 1796	45 32	41 40	+ 3 52
Jan. 28, 1802 -	49 18	44 19	+ 4 59
Auguſt 31, 1802	46 47	44 36	+ 2 11
March 23, 1803	43 43	44 52	− 1 9
March 26, 1803	44 52	44 52	0 0

The real motion from *h* to *h′* is projected into that from *x* to *x′*; and, while the elliptical arch ſubtends an angle of 12° 33′, the circular one will be about 4° 50′.

From the figure of the orbit, we may conclude that the ſmall ſtar, in its firſt poſition, at *x* or *h*, was not more than between 30 and 40 years paſt its conjunction; and that, conſequently, the parts of the arch *x x′*, were nearly proportional to the times of their being deſcribed. The poſitions have been calculated upon this principle; but with ſome allowance for the firſt obſerved angle, which I ſuppoſe to have been a little too ſmall; and, though the differences of the obſerved and calculated angles are pretty conſiderable, the obſervations are ſtill ſufficiently conſiſtent to prove the gradual change of the ſituation of the ſmall ſtar.

The quantity of the change in 22 years and 207 days, will ſhow that a periodical revolution cannot take up leſs than 1681 years. The real figure and ſituation of the orbit, with many other particulars, are ſtill unknown; it is, therefore, unneceſſary to point out the uncertainties in which the inveſtigation of the periodical time of the ſmall ſtar about ε Bootis muſt long remain involved.

ζ *Herculis*.

ζ *Herculis.*

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

My obſervations of this ſtar furniſh us with a phenomenon which is new in aſtronomy; it is, the occultation of one ſtar by another. This epoch, whatever be the cauſe of it, will be equally remarkable, whether owing to ſolar parallax, proper motion, or motion in an orbit whoſe plane is nearly coincident with the viſual ray. My firſt view of this ſtar, as being double, was July 18, 1782. With 460, the ſtars were then $\frac{1}{2}$ diameter of the ſmall ſtar aſunder. The large ſtar is of a beautiful bluiſh white, and the ſmall one aſh-coloured.

July 21, of the ſame year, I meaſured the angle of poſition, 20° 42′ north following. With the ſtandard power, the diſtance of the ſtars remained as before. With 987, they were one full diameter of the ſmall one aſunder.

In the year 1795, I found it difficult to perceive the ſmall ſtar; however, in October of the ſame year, I ſaw it plainly double, with 460; and its poſition was north following.

Other buſineſs prevented my attending to this ſtar till the year 1802, when I could no longer perceive the ſmall ſtar. Sometimes, however, I ſuſpected it to be ſtill partly viſible; and, in September of the ſame year, with 460, the night being very clear, the apparent diſk of ζ Herculis ſeemed to be a little lengthened one way. With the ten-feet teleſcope, and a power of 600, I ſaw the two ſtars of η Coronæ very diſtinctly; and, having in this manner proved the inſtrument to act well, I directed it to ζ Herculis, and found it to have the appearance of a lengthened, or rather wedge-formed ſtar; after which, I took a meaſure of the poſition of the wedge.

Our temperature is ſeldom uniform enough to permit the uſe of very high powers; however, on the 11th of April, 1803, I examined the apparent diſk, with a magnifier of 2140, and found it, as before, a little diſtorted; but there could not be more than about $\frac{3}{8}$ of the apparent diameter of the ſmall ſtar wanting to a complete occultation. Moſt probably, the path of the motion is not quite central; if ſo, the diſk will remain a little diſtorted, during the whole time of the conjunction. Our preſent obſervations cannot determine which of the ſtars is at the greateſt diſtance; but this will occaſion no difference in the appearance; for, if the ſmall ſtar ſhould be the neareſt, its light will be equally loſt in the brightneſs of the large one.

The

The obſervations I have made on this ſtar, are not ſufficient to direct us in the inveſtigation of the nature of the motion by which this change is occaſioned.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

We may however be certain, that with regard to

Single Motions.

(a, b) Neither x nor ζ can be ſuppoſed to be the only moving bodies, without contradicting the highly probable arguments for the ſun's motion.

(c, d) If we admit the ſun to be the moving body, the ſtars ζ and x being at reſt, we may calculate the effect of the ſolar parallax upon them, as follows. Let O move towards λ Herculis, with the annual velocity 1, as in the caſe of α Geminorum; then, from the ſituation and magnitude of the large ſtar of ζ Herculis, which we will ſuppoſe 4m, the effect of the ſolar motion at ζ will be only ,0522; and, at x, ſuppoſed to be at the diſtance 5m, it will be ,0418. This will ſhow itſelf at the parallel of ζ in a direction of 25° 5′ north preceding, the ſolar motion being in the oppoſite direction ſouth following. But this parallax will only produce, in 20 years and 10 months, an apparent change of 0″,444 in ζ, and of 0″,355 in x; and will ſeparate the ſtars, inſtead of bringing them to a conjunction.

(e) A conſiderable advantage may be gained, by placing x at a little more than $\frac{1}{3}$ the diſtance of ζ from O. For as, in the above-mentioned time, this would make the effect of parallax upon it 1″,18, a conjunction ſhould now take place. But then the ſtars, though very near each other, would not be quite in contact; much leſs could one of them occaſion an occultation of the other. The ſuppoſition alſo, that the ſmall ſtar ſhould be only $\frac{1}{3}$ of the diſtance of the large one from us, is not very favourable to the hypotheſis.

δ Serpentis.

This double ſtar has undergone a very conſiderable change in the angle of poſition, but none in the diſtance of the two ſtars. The 5th of September, 1782, an accurate meaſure of the poſition was 42° 48′ ſouth preceding; and February 7, 1802, it meaſured 61° 27′ ſouth preceding. In 19 years and 155 days, therefore, the ſmall ſtar has moved, in a retrograde order, over an arch of 18° 39′.

Every

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

Every argument, to examine the cauſe of this motion, which has been uſed with ε Bootis, in the paragraphs from *(a)* to *(i)*, will completely apply to this ſtar; from this we may conclude, that the moſt natural way of accounting for the obſerved changes, is to admit the two ſtars to form a binary ſyſtem. In this caſe we calculate, with conſiderable probability, that the periodical time of a revolution of the ſmall ſtar round ϑ Serpentis, muſt be about 375 years.

γ Virginis.

This double ſtar, which has long been known to aſtronomers,* has undergone a viſible change ſince the year 1780, when I firſt began my obſervations of it. The 21ſt of November, 1781, I meaſured the poſition of the two ſtars, which was 40° 44′ ſouth following. The ſtars are ſo nearly equal, that I have but lately aſcertained the following one to be rather larger than its companion; the poſition, therefore, ought now to be called north preceding. By a mean of three meaſures, that were taken on the 15th of April, 1803, the angle was 30° 20′ *np*.

The diſtance, as far as eſtimations by the diameter can determine, when the ſtars are ſo far aſunder as theſe are, remains without alteration. May 21, 1781, they were $2\frac{1}{2}$ diameters aſunder; and, by eſtimations lately made, with the ſame inſtrument and power as were uſed 21 years ago, the ſtars are ſtill at the ſame diſtance of $2\frac{1}{2}$ diameters.

A very ſmall proper motion in declination, of 0″,02 towards the ſouth, has been aſſigned to this double ſtar;† but the quantity is hardly ſufficient for us to rely much upon the accuracy of the determination. I ſhall therefore rather conſider γ Virginis as one of the ſtars of which we have no proper motion aſcertained; and the arguments to which I ſhall refer, will conſequently be thoſe which have been given with ε Bootis.

The change of the angle of poſition, in the time of 21 years and 145 days, amounts to 10° 24′; from which we obtain the annual motion of 29′,16. The obſerved and calculated angles, with their differences, on which it will not be neceſſary to make any remarks, are in the following Table.

* *Memoires de l'Academie des Sciences.* Ann. 1720.

† *Connoiſſance des Temps*, Année VI. page 213.

Times

Times of the Obſervations.	Obſerved angles.	Calculated angles.	Differences.
Nov. 21, 1781 -	40° 44′	40° 44′	0° 0′
Jan. 29, 1802 -	28 22	30 51	—2 29
April 15, 1803 -	30 20	30 20	0 0
May 28, 1803 -	32 2	30 17	+1 45

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

As a confirmation of the accuracy of theſe obſervations, we may have recourſe to a poſition of the ſame ſtars, deduced from the places of them, as they are given in Mayer's Zodiacal Catalogue. By two obſervations reduced to the beginning of the year 1756, the preceding one was 3″,8 before the other in right aſcenſion, and 5″,3 more north than that ſtar. From this we calculate the poſition, which was 54° 21′ 37″ north preceding. The interval from the 1ſt of January, 1756, to the 21ſt of November, 1781, is 25 years and 325 days. When this is added to the period I have given, we have 47 years and 105 days, for a motion of 24° 2′. The annual motion, deduced from this lengthened period, which is 30′,5, differs leſs than $1\frac{1}{2}$ minute from that which has been calculated from my obſervations. With the aſſiſtance, therefore, of Mayer's obſervations, which greatly ſupports our calculation, we may conclude, that the two ſtars of γ Virginis revolve round each other in about 708 years.

V.

Account of the Invention of the Compound-Barrel and Winch, and its uſeful Application on a Large Scale. In a Letter from Mr. J. J. Hawkins.

To Mr. NICHOLSON.

SIR,

In No. 25 of your uſeful Journal, p. 50, you mention the compound-barrel and winch, as the invention of the celebrated Mr. George Eckhardt.

Not in the leaſt wiſhing to ſubtract from the well known inventive talents of that Gentleman, I beg leave to ſay, that, if it was invented by him, it was alſo invented to my certain knowledge, by the late Mr. Robert Mc Kean of Philadelphia, ſon to the preſent governor of Pennſylvania.

The compound barrel and winch was alſo invented by Mr. R. Mc Kean.

In 1795 or 1796. Deſcription.

Mr. Mc Kean communicated the idea to me a few days after it firſt occurred to him, in the year 1795 or 1796; his plan differed, however, in two points, from that deſcribed in your Journal; for, firſtly, he made uſe of handſpikes applied into holes in the end of the largeſt barrel, whenever the winch did not afford him ſufficient power; and, ſecondly, when he found it neceſſary to raiſe any thing above the level of his windlaſs, he added two other pullies for the rope to paſs over.

Dimenſions and application of a compound barrel.

About the year 1797, Mr. Mc Kean erected a ſaw-mill worked by a ſteam engine, at Bordenton in New Jerſey; where he employed this crane to raiſe his logs, from the ground to the frame on which they were to be cut, being a height of about twelve feet; the circumference of the largeſt barrel was about ſix feet, and its length three feet; the ſmalleſt barrel was of the ſame length, and its circumference three feet, making the difference of their circumferences three feet; and, conſequently one turn of the barrels, raiſed the log one foot ſix inches; and eight turns only were ſufficient, to elevate it the height required.

Its power as one to twenty.

This windlaſs was turned with handſpikes ſix feet long; which, at the place where the power is generally applied, would deſcribe a circle of about 30 feet; ſo that the power obtained, is as one to twenty. The length of rope neceſſary, including that which paſſes round the pullies, and from thence to the barrels, is about 55 feet.

A common tackle would have required five times as much rope;

but thinner.

Now in order to raiſe a weight 12 feet high, with a power of one to twenty, by means of the common block pullies, no leſs than 270 feet of rope is neceſſary; there is, however, this difference in the ſizes of the ropes, that, in the former caſe ſupporting one half of the weight to be raiſed, whereas in the latter it has only to ſuſtain one twentieth part of it.

This windlaſs does not run down.

The windlaſs has the peculiar property of holding the weight at any part of its riſe or fall, without needing a ratchet-wheel and catch; by reaſon of the two parts of the rope pulling on oppoſite ſides of the barrels; and although one pulls on a longer lever than the other, yet it is not ſufficient to overcome the friction on the axis, which muſt of neceſſity be large, in order to give the requiſite ſtrength to the machine.

It promiſes to be extenſively uſeful.

I have lately introduced a ſmall windlaſs of this kind, into a piece of machinery requiring alternate elevation and depreſſion,

preſſion, and find it extremely convenient; and I have no doubt it will be adopted in a great variety of machinery when its valuable properties are generally known.

I am,

Sir, Yours, &c.

J. J. HAWKINS.

11, *New Liſle Street,*
March 17, 1804.

VI.

Propoſal for conſtructing a Galvanic Apparatus of Great Power, for the Combuſtion of Metals. By a Correſpondent. I. R. I.

To Mr. NICHOLSON.

SIR,

THE following idea of a mode of increaſing to an immenſe degree the galvanic power of inflaming metals, took its origin from Mr. Wilkinſon's paper on that ſubject in your laſt Journal; and I beg that if you think this communication can be of the ſmalleſt uſe to that ingenious gentleman, that you will ſend it to him before publication. Increaſe of galvanic combuſtion.

I can hardly think that it could have eſcaped his penetration, (though I cannot account for his omitting the obſervation) that the natural conſequence of his diſcovery relative to the ratio by which the galvanic trough increaſes in its power of inflaming metals, is, that the maximum of effect will be produced (from a given ſurface,) by one pair only; ſo that the ſame ſurface of metal that in his apparatus at preſent conſtructing and diſtributed into 50 pairs, will burn 108 feet of ſteel wire $\frac{1}{70}$ of an inch thick, if concentrated into one, will have the ſame effect on 5400 feet of the ſame wire; an enormous increaſe of a power that may become as noble a preſent to the arts, from ſcience, as the ſteam-engine has proved. by one ſingle plate or pair.

I have alſo turned my thoughts towards making a new form of apparatus, on the above principle, ſo as to condenſe it into as little bulk as poſſible. The ſimple inſpection of the drawing accompanying this will ſufficiently illuſtrate it; the ſcale is of three inches to a foot, (*Pl.* XV.) it repreſents part of the ground plan of a trough, of about ten feet long, two feet broad, and two and half feet deep: the black line repreſents the copper, the dotted Deſcription of the propoſed apparatus.

the

the zinc, the white the acid liquor, and the ſhaded part the wood: the ſcale is made large for the ſake of diſtinctneſs; two ſpigots or turncocks are adapted to the end by which the liquor may be drawn off; a contrivance of great uſe, but impracticable in the preſent form; only two convolutions of the plates are expreſſed, and the turns are made about three inches wide, to facilitate the ſoldering of the zinc plates to the copper; the liquor is confined in as ſmall a ſpace as poſſible, to prevent the unneceſſary waſte of acid. It may alſo be conſtructed with one metal only, on Mr. Davy's plan; in that caſe, the ſulphuret will occupy one ſide of the plate, and the acid the other. Hoping that the above may merit a place in your very excellent Journal, and be of uſe to my fellow labourers in ſcience,

I remain, Sir,

Your moſt obedient humble Servant,

I. R. I. an old Correſpondent.

Edinburgh, March 13, 1804.

VII.

A Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattle-Snake and other American Serpents. By Benjamin Smith Barton, M. D*. *From the American Tranſactions,* Vol. IV.

Fidem non abstulit error.

Naturaliſts frequently too credulous.

NATURALISTS have not always been philoſophers. The ſlight and ſuperficial manner in which they have examined

* Since this memoir was read before the Society, it has been conſiderably altered, and ſomewhat enlarged. I hope, the alterations will render it more worthy of the notice of thoſe who, like myſelf, derive pleaſure and happineſs from the contemplation of the works and operations of nature, on this globe.

I fear, I ſhall be thought to have treated the queſtion in too diffuſive a manner. I have not, indeed, laboured to be conciſe. But if the memoir is more extenſive than was neceſſary, I flatter myſelf, it will be admitted that it, at leaſt, contains ſome new and intereſting facts. I ſubmit it to its fate. B. S. B.

This intereſting paper has been deferred from the preſſure of other matter of more immediate and temporary claim. W. N.

many

many of the ſubjects of their ſcience; the credulity which has accompanied them in their reſearches after truth, and the precipitancy with which they have decided upon many queſtions of importance, are proofs of this aſſertion.

Faſcination by ſnakes.

There is a queſtion in natural hiſtory that has, in an eſpecial manner, ſolicited from me theſe obſervations. I mean the queſtion concerning the faſcinating faculty, which has been aſcribed to different kinds of American ſerpents. It is my intention to examine this queſtion, in the memoir which I now preſent to the Philoſophical Society.

generally believed in America.

Of this faſcinating faculty we have all heard and read. In many of our country ſituations, there is hardly a man or a woman, who will not, when the ſubject comes to be mentioned, ſeriouſly relate ſome wonderful ſtory, as a convincing proof of the doctrine. Children ſeem taught to believe it. I think, it is ſometimes one of the earlieſt prejudices imprinted on their tender minds. It is a prejudice which often increaſes with their years; and even in that happy period of life when the mind is moſt firm, and the leaſt propenſe to the belief of extraordinary things, the ways of which we are not capable of ſcanning, I have known this prejudice ſo deeply and ſo powerfully rooted, as to mock the light and ſureneſs of facts, and all the ſtrength of reaſoning.

Examination of the queſtion propoſed.

It is not my intention, in this memoir, to give an analyſis, or complete view, of every thing that has been written on the ſubject. Nor is it my intention to examine the many ſtories, related by authors, in ſupport of the faſcinating faculty of ſerpents. For the firſt taſk, I have not leiſure; and, as to the ſecond, I ſhould think my time ill employed in pointing out the groſs abſurdities which ſeem to conſtitute a neceſſary part of many of thoſe ſtories. I think it proper, however, to obſerve, that I have anxiouſly ſought for, and have patiently peruſed, the volumes of tales publiſhed in favour of the doctrine which I mean to controvert.

General view.

I aim at giving a general, though correct, view of the queſtion, uninfluenced by the bold aſſertions of ignorance, or by the plauſible conjectures of ſcience. In the inveſtigation of the queſtion, I have ſought for facts: theſe have been my guides. I have ſtudiouſly endeavoured to follow where they ſeemed to lead. Perhaps, they have led me aſtray.

The

The manner in which the ſuppoſed faſcinating power of ſerpents is exerted has often been related, by different writers. I ſhall endeavour to convey ſome idea of the buſineſs, in as few words as I can.

Deſcription of the ſuppoſed proceſs of ſnakes in faſcinating other animals.

The ſnake, whatever its ſpecies may be, lying at the bottom of the tree or buſh upon which the bird or ſquirrel ſits, fixes its eyes upon the animal which it deſigns to faſcinate or enchant. No ſooner is this done than the unhappy animal (I uſe, for the preſent, the language of thoſe who differ from me in opinion, on this ſubject) is unable to make its eſcape. It now begins to utter a moſt piteous cry, which is well known by thoſe who hear it, and underſtand the whole machinery of the buſineſs, to be the cry of a creature enchanted. If it is a ſquirrel, it runs up the tree for a ſhort diſtance, comes down again, then runs up, and, laſtly, comes lower down. "On that occaſion," ſays an honeſt but rather credulous writer *, "it has been obſerved, that the ſquirrel always goes down more than it goes up. The ſnake ſtill continues at the root of the tree, with its eyes fixed on the ſquirrel, with which its attention is ſo entirely taken up, that a perſon accidentally approaching, may make a conſiderable noiſe, without the ſnake's ſo much as turning about. The ſquirrel as before mentioned comes always lower, and at laſt leaps down to the ſnake, whoſe mouth is already wide open for its reception. The poor little animal then with a piteous cry runs into the ſnake's jaws, and is ſwallowed at once, if it be not too big; but if its ſize will not allow it to be ſwallowed at once, the ſnake licks it ſeveral times with its tongue, and ſmoothens it, and by that means makes it fit for ſwallowing †."

Other accounts are not materially different.

It would be eaſy to cite, from different authors, other accounts of the manner in which the enchantment is performed; or, more properly ſpeaking, of the conduct, or behaviour, of the enchanting and enchanted animals. But between theſe accounts, there is hardly a ſpecific difference. There is conſiderable unity in all the relations that I have heard, or read.

* Profeſſor Peter Kalm.

† Travels into North-America; containing its natural hiſtory, and a circumſtantial account of its plantations and agriculture in general, &c. &c. vol. i. p. 317 & 318. Alſo vol. ii. p. 207, 208, 209 & 210. Engliſh Tranſlation. London: 1770 & 1771.

However,

However, thoſe who wiſh to examine this part of the ſubject more fully, will, at leaſt, receive ſome degree of entertainment from the peruſal of the many authors who have believed and aſſerted, that ſerpents poſſeſs a power of faſcinating other animals.

Credulity in the vulgar is to be expected;

That the belief in the exiſtence of this power ſhould have been ſo general among the uninformed part of a people, ought not to be wondered at. The human mind, unenlightened by ſcience, or by conſiderable reflection, is a ſoil rich in the weeds of ſuperſtition, and credulity. It is ever prone to believe in the wonderful, even when this belief, as is often the caſe, brings with it fears, and cares, and miſery. The bondage of the mind in ſuperſtitious credulity is great and heavy. Neither religion nor virtue can give it its freedom. This it obtains from ſcience. How important, then, even in this point of view, is the enlargement of the mind by ſcience!

but it is ſtrange that men of ability have alſo admitted theſe facts.

But it is, ſurely, a matter of ſome aſtoniſhment, that this belief ſhould have been admitted, in all the fulneſs of its extravagance, by men of learning, of obſervation, and of genius: by thoſe who have the book of nature in their hands; that book which will, in ſome future and ſome happier age, eradicate many of the prejudices which diſfigure, and which mock the dignity of human nature: by claſſical ſcholars, grown old in the diſbelief of ſimilar fables, heightened and embelliſhed by the charms of poetry; and alſo by the infidel, who denies the authenticity of ſcripture-miracles, few of which, even though they were not ſhown to be truths, are more improbable than the imaginary fact which I am examining.

This notion is not found in the writings of the ancients.

I have ſought to diſcover the original, or ſource of this belief. I do not find any traces of it among the ancient writers of either Greece or Rome. I think, it is moſt likely that no ſuch traces can be found. Lucan, had ſerpents been thought to poſſeſs a faſcinating faculty in his age, and in the country in which he lived, would, probably, have availed himſelf of its exiſtence, in his beautiful account of the march of Cato's army through the Libyan-Deſert*; and had ſuch a notion prevailed in the earlier days of Lucretius, would we not find ſome mention made of it in the poem *De Rerum Natura*, one of the fineſt and moſt varied productions of the human mind? Claſſical ſcholars may poſſibly, however, diſcover the dawn

* Pharſalia, lib. IX.

of this notion in Greek and Roman authors, unread by me. On this ſubject, I have not puſhed my inquiries as far as I wiſhed to have done. It is not unlikely that I may examine the queſtion more curiouſly, at ſome future period.

It is probable that in the mythology of Aſia and of Africa, we ſhall diſcover ſome traces of this notion, ſo intimately connected with the ſuperſtitious credulity of a people, and even ſo naturally ariſing out of an imperfect view of the manners of ſerpents.

It does not appear to have originated nor to be much, if at all, admitted among the Indians.

If we believe the Reverend Dr. Cotton Mather *, Mr. Dudley †, and other perſons, who had reſided in North-America, we are to look for the beginning of this ridiculous notion among our Indians. How far, however, this is really the caſe may, I think, be doubted. It is certain that, at preſent, the opinion is by no means univerſal among the Indians. Several intelligent gentlemen, who are well acquainted with the manners, with the religious opinions, and with the innumerable ſuperſtitious prejudices of the Indians, have informed me, that they do not think theſe people believe in the notion in queſtion. My friend Mr. John Heckewelder, of Bethlehem, writes to me, that he does not recollect to have heard the Indians ſay that ſnakes charm birds; though he has frequently heard them ſpeak of the ingenuity of theſe reptiles in catching birds, ſquirrels, &c. Mr. William Bartram ſays, that he never underſtood that the nations of Indians among whom he travelled had any idea of the faſcinating power of ſnakes ‡. On the other hand, however, a Mohegan-Indian told me that the Indians are of opinion that the rattle-ſnake can charm, or bewitch, ſquirrels and birds, and that it does this with its rattle, which it ſhakes, thereby inviting the animals to deſcend from the trees, after which they are eaſily caught. According to this Indian, his countrymen do not think that the ſnake, in any manner, accompliſhes the buſineſs with its eyes. A Choktah-Indian aſſured me that the rattle-ſnake does charm birds, &c. but he was honeſt enough to confeſs that he

* The Philoſophical Tranſactions, abridged, vol. v. part ii. no. 339. p. 162.

† Ibid. vol. vi. part iii. no. 376. p. 45.

‡ MS. note, communicated to me by this ingenious gentleman.

did

did not know in what manner it does it. The interpreter, through whom I converſed with this Indian, ſaid that the ſnake charms by means of its rattle.

Some North-American tribes had a veneration for the rattle-ſnake;

The veneration, or regard, which has been paid to the rattle-ſnake by certain North-American tribes ſeems, at firſt ſight, to favour the opinion, that theſe tribes attributed to this hideous reptile ſome hidden power *, perhaps that of faſcinating animals. Mr. William Bartram informs me, that the ſouthern Indians, with whom he is acquainted, ſeem to hold the rattle-ſnake in a degree of veneration †. Mr. Heckewelder ſays that, to his certain knowledge, this reptile was once held in particular eſteem by the Delawares. He was ſeveral times prevented, by theſe Indians, from killing the rattle-ſnake, being told that it was their grand-father, and, therefore, muſt not be hurt. At other times, he was told, he muſt not kill this ſnake, becauſe the whole race of rattle-ſnakes would grow angry, and give orders to bite every Indian that might come in their way ‡. But, of late, eſpecially among thoſe Indians who have had connection with the whites, theſe ridiculous notions have mouldered away, and our Indians, at preſent, kill their rattling "grandfather" with as little ceremony as the Eſkemaux are ſaid to kill their parents in old-age.

which ſeems to have been part of the religious ſyſtem grounded on fear.

It is obvious, from contemplating the manners and the hiſtory of nations, that a part of their religions, and a large part of the fabrick of their ſuperſtitious notions, have ariſen

* Vis abdita. Lucretius.

† MS. note communicated to me.

‡ In my *Hiſtorical and Philoſophical Inquiry* (not yet publiſhed), I have collected many facts which ſeem inconteſtibly to prove, that the mythology, or ſuperſtitious religion, of the Americans is a fragment of that mythology whoſe range in Aſia, and in Africa, has been ſo extenſive. Poſſibly, the veneration, or regard, which was paid to different kinds of ſerpents in America did not originate in this continent, but had its ſource in Aſia, from which portion of the globe (after a long and laborious attention to the ſubject) I cannot doubt, that almoſt all the nations of America are derived. It is unneceſſary, in this place, to cite inſtances of the religious veneration which was, and ſtill is, paid to ſome ſpecies of ſerpents, in various parts of the old-world. Theſe inſtances muſt be familiar to every perſon, who is acquainted with the hiſtorians or with the poets of antiquity, and with the hiſtory of the Gentoo-Indies.

out of fear. Perhaps, all mankind * admit the exiſtence of two great beings, the one good and all-benevolent, the other bad and ſtudious of evil. In our own continent, where, I believe, this notion was univerſal, certain tribes were aſſiduous in their adoration of the latter being, whilſt the former, whom the light of reaſon taught them to conſider as the ſource of life, and all their bleſſings, was merely acknowledged and named, but unworſhipped and neglected †. The Delawares, and ſome other nations who ſpeak dialects of their language, believe that a turtle, of an enormous ſize, inhabits the deep, and ſupports upon his back this continent, or, as they call it, iſland. They ſay it is in the power of this animal, by diving, to drown the world, as he has already done, in former ages. They, therefore, endeavour to conciliate his friendſhip and good-will. With this view, they make rattles of the turtle-ſhell, into which they put ſmall ſtones, beans, or Indian-corn ‡, and play with this inſtrument, at their dances. The turtle is greatly eſteemed by them; and, in the fulneſs of a mixed zeal and fear, they even deign to call him *Mannitto*, or God; becauſe, they ſay, he can live both upon the land and in the water §.

It is not likely that they venerated the ſnake from a belief of its faſcination.

It ſeems very probable to me, that the veneration for the rattle-ſnake had its birth in fear, and not in the belief that this reptile poſſeſſed the power of faſcinating animals. If, as ſome writers have aſſerted, the Indians were in poſſeſſion of abſolute ſpecifics for the bite of the rattle-ſnake, I am of opinion that the veneration for this animal would not have exiſted; or, at leaſt, that it would not long have continued. But the Indians are often unable to prevent or to cure the effects of the active poiſon of this ſerpent, which not unfrequently deſtroys them ‖.

I return

* I ſpeak of mankind in the aggregate, and not of individuals among them.

† John De Laet, ſpeaking of the Indians of New-York, has the following words: "Cæterum nullus ipſis religionis ſenſus, nulla Dei veneratio: diabolum quidem colunt ſed non tam ſolemniter neque certis ceremoniis, ut Africani faciunt," &c. Novus Orbis ſeu Deſcriptionis Indæ Occidentalis Libri xviii. lib. iii. cap. xi. p. 75. Lugd. Batav. 1633.

‡ Maize.

§ MS. by Mr. John Heckewelder, *penes me.*

‖ Adair ſays, he does "not remember to have ſeen or heard of an

I return to the more immediate path of my ſubject.

The South Americans do not poſſeſs this notion,

Among the Indians of South-America, I do not find any traces of the notion that ſerpents can faſcinate other animals. Piſo, the author of the *Natural and Medical Hiſtory of the two Indies*, ſeems to have been ſtudious to bring together the extraordinary things which have been related of the rattle-ſnake. But he ſays not a ſyllable concerning the faſcinating faculty of this reptile *.

but it has ſpread over all America and Europe.

But whatever may have been the native country of the notion which I am conſidering, it would have been well had it been confined to ſavages. It is a tale which ſeems nicely adapted to the wit and ſociety of rude and uncultivated nations. Unfortunately, the progreſs of error and credulity is extremely rapid. Their dominion is extenſive. The belief in the faſcinating faculty of ſerpents has ſpread through almoſt all the

Indian dying by the bite of a ſnake, when out at war, or hunting; although they are then often bitten by the moſt dangerous ſnakes." *The Hiſtory of the American Indians*, &c. p. 235. London: 1775. It is certain, from the teſtimony of many perſons, that the bite of the rattle-ſnake has often proved mortal to the Indians, and others, notwithſtanding the boaſted ſpecifics of theſe people. Father Cajetan Cattaneo ſays, many Indians die with the bite of ſerpents. "But," obſerves the father, "it is ſaid they commonly eſcape with life, when they can quickly apply the remedy which providence has prepared of certain herbs, eſpecially the ſpikenard, which ſome parts of *Paraguay* produce in plenty. But when they are bit by the rattle-ſnake it is confidently aſſured that the caſe admits no cure." *The third letter of F. Cajetan Cattaneo.* See *A Relation of the miſſions of Paraguay, wrote originally in Italian, by Mr. Muratori.* Engliſh Tranſlation. p. 260. London: 1759. Father Cattaneo is here ſpeaking of the South-American rattle-ſnake, the poiſon of which, I have little doubt, is more deleterious than that of the ſame animal in our part of North-America. Still, however, I am confident, that this poiſon, even in the moſt fervid climates, is not always mortal.

* Gulielmi Piſonis medici Amſtelædamenſis de Indiæ utriuſque re naturali et medica libri quatuordecim. Amſtelædami: apud Elzevirios, 1658. Some of Piſo's aſſertions concerning the rattle-ſnake are very extravagant. Such are the following: "Caudæ extremitate in anum hominis immiſſa, mortem infert confeſtim; venenum autem quod ore vel dentibus infundit, multo lentius vitam tollit." p. 275.

civilized

civilized parts of North-America. Nor is it confined to America. It has made its way into Europe, and has there taken poſſeſſion of the minds of ſcholars, of naturaliſts, and of philoſophers.

It is ſaid that Dr. Johnſon believed it. But, he was credulous, timid, and melancholy.

I think, I have ſomewhere either heard or read that the tale was credited by the late Dr. Samuel Johnſon. If I am miſtaken, I hope the admirers of this great man, ſhould any of them read my memoir, will pardon me. It is certain, notwithſtanding the vaſt ſtrength and the rich fertility of Johnſon's mind, that he was credulous and timid. Did this union of credulity and timidity ariſe out of that unhappy melancholy ("thoſe caſual eclipſes which darken learning",) that often overclouded the brightneſs of the mind*? We are told that the Hercules of Engliſh literature believed in ghoſts, and in the ſecond-ſight. The man who would thus ſuffer his mind to be eſtranged from probability, and entangled in difficulties, would, perhaps, find it eaſy to bend to the belief, that ſerpents have the faculty of faſcinating other animals.

Linnæus admits it.

Although I profeſs myſelf to be a warm admirer of Linnæus, and although, at a very early period of my life, I enliſted myſelf under the banner of his ſchool, I ſhall not, nevertheleſs, attempt to conceal, that this great man gave credit to the tale of the faſcination of birds and other animals by ſerpents. In his *Syſtema Naturæ* (that immortal work), under the article Crotalus horridus, or the rattle-ſnake, he has the following words: "*Aves Sciuroſque ex arboribus in fauces revocat.*"†. In another work, he ſpeaks as follows. "Whoever is wounded by the Hooded Serpent *(Coluber Naja)* expires in a few minutes; nor can he eſcape with life who is bitten by the rattle-ſnake, *(Crotalus horridus)* in any part near a great vein. But the merciful God has diſtinguiſhed theſe peſts by peculiar

* Or, did his melancholy grow out of his credulity and fear?

† See volume firſt, p. 372. Vienna edition of 1767. Profeſſor Gmelin, in his edition of the *Syſtema Naturæ*, when ſpeaking of the rattle-ſnake, has the following words, viz. "*aves ſciurique ex arboribus non raro in fauces inhiantis apertas incidunt,*" tom. i. pars iii. p. 1080. The ſame laborious author ſpeaking of our grey-ſquirrel (Sciurus cinereus) ſays, "a crotalo comeditur," tom. i. p. 147. This is true: but he might have ſaid the ſame when ſpeaking of the ſtriped-dormouſe, or ground-ſquirrel (Sciurus ſtriatus), of our rabbit (Lepus americanus), and many other animals.

ſigns,

ſigns, and has created them moſt inveterate enemies; for as he has appointed cats to deſtroy mice, ſo has he provided the Ichneumon *(Viverra Ichneumon)* againſt the former ſerpent, and the Hog to perſecute the latter. He has moreover given the *Crotalus* a very ſlow motion, and has annexed a kind of rattle to its tale, by the motion of which it gives notice of its approach: but, leſt this ſlowneſs ſhould be too great a diſadvantage to the animal itſelf, he has favoured it with a certain power of faſcinating ſquirrels from high trees, and birds from the air into its throat, in the ſame manner as flies are precipitated into the jaws of the lazy toad."* His account.

Linnæus was, certainly, extremely credulous, though I do not find that any of his profeſſed biographers have taken notice of this feature of his mind. But the proofs of my obſervation are numerous: they are to be found in almoſt every eſſay that he has written. His credulity with reſpect to the powers of medicines is, perhaps, peculiarly ſtriking †. How far this credulity, in a mind otherwiſe truly great (a mind which with reſpect to the arrangement of natural bodies has never been equalled), is to be ſought for in the general character of the country which gave Linnæus birth, I ſhall not pauſe to inquire. Yet in an inveſtigation of this kind, where the opinion of the Swediſh Pliny is neceſſarily mentioned, it might be curious to look to the ſources of his credulity. The ſtudy of nature, as it reſpects this globe, is, perhaps, of all the ſciences, the moſt unfavourable to ſuperſtition, or credulity. But the greateſt of naturaliſts was one of the moſt credulous of philoſophers. It is certain that Linnæus was credulous.

It is proper, however, to obſerve, in this place, that Linnæus by no means aſſerts, that he himſelf had ever been a He never was himſelf a witneſs to the fact.

* See Reflections on the Study of Nature, tranſlated from the Latin of Linnæus. p. 33 & 34. Dublin edition, 1786. Dr. I. E. Smith, the ingenious tranſlator of this diſſertation, in a note to the above paſſage, has the following words. "This opinion of the faſcinating power of the Toad has been refuted, and the appearance which gave riſe to it fully accounted for, by Mr. Pennant, in his Britiſh Zoology. Probably the ſtory of the rattle-ſnake's having a ſimilar power might be found equally falſe, if enquired into with the ſame degree of accuracy." p. 34.

† See his Materia Medica, liber. i. de Plantis, &c. Amſtelædami: 1749.

witneſs

witneſs to the faſcinating power of any of the ſerpent-tribe. He ſeems to have received the tale from ſome of his many pupils, whom he animated with the love of natural hiſtory. It is probable that Kalm, whom Linnæus quotes upon various occaſions, and whom he could not but eſteem, principally contributed to fix his illuſtrious maſter's credulity in this reſpect: for, in different parts of his *Travels*, this induſtrious author has given his decided aſſent to the tale; and he informs us, that he has treated of the ſame ſubject, more fully, in a treatiſe which is printed in the *Memoirs of the Royal Swediſh Academy of Sciences*, for the year 1753 *.

Nor was Kalm; though he ſupports it,

Kalm is candid enough to tell us, that he never ſaw an inſtance of the faſcinating power of the ſerpent-kind. "However," ſays he, "I have a liſt of more than twenty perſons, among which are ſome of the moſt creditable people, who have all unanimouſly, though living far diſtant from each other, aſſerted the ſame thing †." He then goes on to tell us a long ſtory, ſimilar to that which I have related, in the beginning of this memoir, and which, therefore, it is not neceſſary to repeat, in this place.

and ſpeculates upon it.

Our author is not content to make mere mention of the fact; he undertakes to ſpeculate upon it. And here, although a talent for ingenious reaſoning is, certainly, not the moſt ſtriking feature that is diſplayed in the *Travels* of Kalm, he acquits himſelf, for ſome time, very judiciouſly; but ſpoils all he has ſaid, by concluding, that the bird or ſquirrel "are only *enchanted*, whilſt the ſnake has its eyes fixed on them.‡" He allows that "this looks odd and unaccountable, though," ſays he, "many of the worthieſt and moſt reputable people have related it, and though it is ſo univerſally believed here," that is in New-Jerſey, &c. "that to doubt it would be to expoſe one's ſelf to general laughter §."

Several American writers have adopted the notion, that ſnakes are endued with a faſcinating faculty. Fearful that their authority may extend the empire of this error, I have been the more anxious to offer my ſentiments on the ſubject to the ſociety ‖.

It

* Travels into North-America, &c. vol. i. p. 318 & 319.

† Ibid. vol. ii. p. 207 & 208.

‡ Travels into North-America, &c. vol. ii. p. 210.

§ Ibid.

‖ Speaking of the rattle-ſnake, my worthy friend Mr. William Bartram

Reſpectable European naturaliſts doubt it.

It has given me pleaſure to find, that the enchanting faculty of the rattle-ſnake is doubted by ſome very reſpectable European naturaliſts. "It is difficult," ſays my excellent friend Mr. Pennant, "to ſpeak of its faſcinating powers: authors * of credit deſcribe the effects. Birds have been ſeen to drop into its mouth, ſquirrels deſcend from their trees, and leverets run into its jaws. Terror and amazement ſeem to lay hold on theſe little animals: they make violent efforts to get away, ſtill keeping their eyes fixed on thoſe of the ſnake; at length, wearied with their movements, and frightened out of all capacity of knowing the courſe they ought to take, become at length the prey of the expecting devourer, probably in their laſt convulſive motion †."

The ſubject treated by la Cépède.

My friend Mr. de la Cépède, one of the moſt eloquent naturaliſts of the age, has devoted a good deal of attention to the ſubject, in his *Hiſtoire Naturelle des Serpens*, a work of extenſive and ſuperior merit. I regret, however, that this ingenious author was not in poſſeſſion of a few facts, well known in this country, which could not have failed to conduct a mind, like his, ſtrengthened by the enlarged contemplation of the objects of nature, to the fulneſs and certainty of truth. As it is, however, Mr. de la Cépède deſerves our thanks for reviving, and giving a new turn to, the ſpeculations of naturaliſts on this ſubject.

Bartram ſays: "They are ſuppoſed to have the power of faſcination in an eminent degree, ſo as to inthral their prey. It is generally believed that they charm birds, rabbits, ſquirrels, and other animals, and by ſtedfaſtly looking at them, poſſeſs them with infatuation; be the cauſe what it may, the miſerable creatures undoubtedly ſtrive by every poſſible means to eſcape, but alas! their endeavours are in vain, they at laſt loſe the power of reſiſtance, and flutter or move ſlowly, but reluctantly towards the yawning jaws of their devourers, and creep into their mouths, or lay down and ſuffer themſelves to be taken and ſwallowed." Travels through North and South Cardinia, Georgia, Eaſt and Weſt Florida, &c. p. 267. Philadelphia: 1791.

* "Lawſon—Cateſby—Ph. Tr. abridg. ix. 56, &c. vii. 410.—Brickel's Hiſt. Carolina, 144.—Beverley Virginia, 260.—Colden, i. 12. Dr. Brickel is an author of no credit. His *Hiſtory of North-Carolina*, here quoted is one of the moſt daring and ſcandalous inſtances of plagiariſm I am acquainted with.

† Arctic Zoology, vol. ii. p. 338. London: 1792.

I beg

I beg leave, in this place, to quote that part of Mr. de la Cépède's work which relates to the queſtion of my memoir.

His account. Suppoſition of infection in the breath.

Speaking of the boiquira, or rattle-ſnake, my ingenious friend has the following words: "His infectious breath, which ſometimes agitates the little animals he is about to ſeize, may alſo prevent their eſcape. The Indians relate, that a rattle-ſnake is often ſeen, curled round a tree, darting terrible glances at a ſquirrel, which after expreſſing its fear by its cries and its tremour, falls at the foot of the tree, where it is devoured. Mr. Voſmaer (at the Hague), who has made ſeveral experiments on the bite of a rattle-ſnake, which he had alive, ſays that the birds and mice, which were thrown into the cage, would immediately endeavour to ſquat in a corner, and that ſoon after, as if ſeized with deadly anguiſh, they would run towards their enemy, who continually ſhook his rattles: but this effect of a mephitick and fetid breadth has been ſo much exaggerated, and miſrepreſented, that it becomes miraculous.

Faſcinating property ſuppoſed to ariſe from this cauſe.

"It has been ſaid," continues our author, "that the rattle-ſnake had a faculty of enchanting, as it were, the animal he intended to devour; that by the power of his glance, he could oblige the victims to approach by ſmall degrees, and finally to fall into his mouth; that even man could not reſiſt the magic force of his ſparkling eyes; and that under violent agitations he would expoſe himſelf to the envenomed tooth of the ſerpent, inſtead of endeavouring to eſcape. If the rattle-ſnake had been more generally known, and if his natural hiſtory had engaged more attention, other circumſtances, ſtill more extraordinary, would have been added to theſe miraculous feats; and how many fables would not have been ſubſtituted to the ſimple effect of a peſtilential breath, which, however, has by no means been either ſo frequent or ſo fatal as ſome naturaliſts have believed.

Or that the animal may have been already bitten.

"We may preſume, with Kalm, that, for the moſt part, when a bird, a ſquirrel, or any other animal, has been ſeen precipitating itſelf from the top of a tree into the jaws of a rattle-ſnake, it had been already bitten *; that after eſcaping, it

* I do not find that Kalm has adopted this ſyſtem of explanation, in his *Travels*. On the contrary, in this work, he gives ſome judicious reaſons for rejecting this mode of explanation. *Travels*, &c.

manifeſted, by its cries and its agitation, the violent action of the poiſon left in its blood, and diffuſed through its circulation, by the envenomed inoculation of the reptile's tooth; that, its ſtrength gradually decaying, it would fly or leap from branch to branch, till finally exhauſted it would fall before the ſerpent, who with inflamed eyes, and eager looks, would watch attentively every motion, and then dart on his prey, when it retained but a ſmall portion of life. Several obſervations related by travellers, and particularly a fact mentioned by Kalm, appeared to confirm this *."

Examination of theſe doctrines.

From this long quotation, it appears that Mr. de la Cépède adopts two modes, or circumſtances, for explaining the miraculous power, which has been attributed to theſe ſerpents. The explanation is, undoubtedly, in both caſes, ingenious, and entitled to notice. I ſhall examine the queſtion with that attention which it deſerves.

In the firſt place, my learned friend ſuppoſes, that the rattle-ſnake's infectious breath †, by agitating the little animals which it means to devour, may prevent their eſcape.

Obſervation to ſhow that ſnakes do not emit any offenſive odour,—

I do not altogether underſtand this expreſſion of an infectious breath. I do not think that we are in poſſeſſion of any facts by which it can be proved, that the breath of the rattle-ſnake is, in general, more infectious, or peſtiferous, than that of many other animals, whether of the ſame or of a different family. I know, indeed, that in ſome of the larger ſpecies

&c. vol. ii. p. 209 & 210. His memoir, in the *Swediſh Tranſactions*, I have not ſeen. Sir Hans Sloane, a long time ſince, conjectured, that the whole myſtery of the faſcinating faculty of the rattle-ſnake is this, viz. "that when ſuch animals as are the proper prey of theſe ſnakes, as ſmall quadrupeds, birds, &c. are ſurpriſed by them, they bite them, and the poiſon allows them time to run a ſmall way; or perhaps a bird to fly up into the next tree, where the ſnakes watch them, with great earneſtneſs, till they fall down, or are perfectly dead, when having licked them over with their ſpawl or ſpittle, they ſwallow them down." *Philoſophical Tranſactions*, vol. xxxviii. no. 403. M. de la Cépède does not appear to have ſeen Sloane's paper.

* Hiſtoire Naturelle des Serpens, p. 409, 410 & 411. a Paris: 1789.

† His words are, "ſon haleine empeſtée, qui trouble quelquefois les petits animaux dont il veut ſe ſaiſir, peut auſſi empêcher qu'ils ne lui échappent." p. 409.

of

of ſerpents, inhabiting South-America, and other countries, there is evolved in the ſtomach, during the long and tedious proceſs of digeſtion in theſe animals, a vapour, or a gas, whoſe odour is intenſely fetid. I have not, however, found that this is the caſe with the rattle-ſnake, and other North-American ſerpents, that I have examined. But my own obſervations on this head have not been very minute. I have made inquiry of ſome perſons (whoſe prejudices againſt the ſerpent-tribe are not ſo powerful as my own), who are not afraid to put the heads and necks of the black-ſnake, and other ſerpents that are deſtitute of venomous fangs, into their mouths, and have been informed, that they never perceived any diſagreeable ſmell to proceed from the breath of theſe animals. I have been preſent at the opening of a box which contained a number of living ſerpents; and although the box had been ſo cloſe as to admit but a very ſmall quantity of freſh air, although the obſervation was made in a ſmall warm room, I did not perceive any peculiarly diſagreeable effluvium to ariſe from the bodies of theſe animals. I am, moreover, informed by a member of this ſociety *, who, has, for a conſiderable time, had a rattle-ſnake under his immediate care, that he has not obſerved that any diſagreeable vapour proceeds from this reptile. On the other hand, however, it is aſſerted by ſome creditable perſons of my acquaintance, that a moſt offenſive odour, ſimilar to that of fleſh, in the laſt ſtage of putrefaction, is continually emanating from every part of the rattle-ſnake, and ſome other ſpecies of ſerpents. This odour extends, under certain circumſtances, to a conſiderable diſtance from the body of the animal. Mr. William Bartram aſſures me, that he has obſerved "horſes to be ſenſible of, and greatly agitated by, it at the diſtance of forty or fifty yards from the ſnake. They ſhowed," he ſays, "their abhorrence, by ſnorting, winnowing, and ſtarting from the road, endeavouring to throw their riders, in order to make their eſcape †." This fact related by a man of rigid veracity, is extremely curious; and, in an eſpecial manner, deſerves the attention of thoſe writers, who, like Mr. de la Cépède, imagine that this fetid emanation from ſerpents is capable of affecting birds,

and others to ſhow that they do.

* Mr. Charles Wilſon Peale.

† MS. note, communicated to me.

at ſmall diſtances, with a kind of aſphyxy *. It even gives *ſome* colour of probability to the ſtory related by Metrodorus, and preſerved in the *Natural Hiſtory* of Pliny †.

(To be continued.)

VIII.

Extract of a Letter from Brugnatelli on the Pruſſiate of Potaſh, and on the Preparation of a fulminating Muriate of Silver. *

IF alcohol be poured into a ſolution of pruſſiate of potaſh (oxipruſſiate of potaſh,) the liquor becomes turbid, and a precipitate is depoſited in the form of ſmall and very brilliant lamellæ, reſembling the ſublimed boracic acid (oxiboracic.) Collecting this precipitate on a filter, and drying it, the ſcales adhere to one another, and form a ſtratum of a ſhining yellowiſh white, like that of mother-of-pearl. This maſs eaſily ſeparates from the filter; is ſoluble in ſulphuric acid (oxiſulphuric), and this ſolution takes a blue colour on the addition of water. Expoſed to the fire in a crucible, it ſwells, burns, and leaves a coally reſiduum, containing a great deal of iron, attractable by the magnet and potaſh. This coally reſiduum, treated with acids, gave out a ſtrong ſmell of ſulphurated hidrogen gas *(gas phlogogène ſulfuré.)*

Alcohol occaſions a precipitate in ſolution of pruſſiate of potaſh, which is ſmall ſcales, adhering to each other, of a ſhining, yellowiſh white, ſoluble in ſulphuric acid; and this ſolution is turned blue by water.

Its coally reſiduum contains a great deal of magnetic iron, and potaſh; and with acids ſmells ſtrongly of ſulphurated hidrogen gas.

I have applied the proceſs I mentioned to you in my laſt to a great number of other metallic oxides (thermoxides), in hope of rendering them fulminating; but I ſucceeded with none, except that of ſilver.

Take a hundred grains of lunar cauſtic (fuſed oxiſeptonate of ſilver) in powder, put them into a beer glaſs, and pour on them firſt an ounce of alcohol, and then as much concentrated nitrous acid *(oxiſeptoneaux.)* The mixture grows hot, enters into a ſtate of ebullition, and an ether is viſibly formed, that changes into a gas. By degrees the liquor becomes milky and opake, and is filled with ſmall and very white clouds. When all the gray powder of the lunar cauſtic has taken this form, and the liquor has acquired a conſiſtency, diſtilled water muſt

On 100 grains of nitrate of ſilver powdered was formed firſt, alcohol 1 ounce, then nitrous acid 1 ounce.

As ſoon as the precipitate was completely formed, water was poured on, and the precipitate dried in a filter.

* Hiſtoire Naturelle des Serpens, p. 355.

† Lib. xxviii. cap. 14.

‡ Van Mons's Journal, IV. 235.

be

be added immediately, to ſuſpend the ebullition, and prevent the matter from being rediſſolved, and becoming a mere ſolution of ſilver. The white precipitate is then to be collected on a paper filter, and dried.

Its weight more than 50 grains. It detonates more ſtrongly than fulminating mercury, being touched with a glaſs tube wetted with ſulphuric acid,

This precipitate is fulminating ſilver, and amounts to more than half the weight of the lunar cauſtic employed. The detonating power of this preparation greatly exceeds that of fulminating mercury, prepared in Howard's or my way, even in much ſmaller quantity. It detonates in a tremendous manner, on being ſcarcely touched with a tube of glaſs, the extremity of which has been dipped in concentrated ſulphuric acid *(oxi-ſulfurique)*, even in that of the ſhops. One grain of this fulminating ſilver, placed on a lighted coal, gave ſuch a loud report, that it deafened the ears of the perſons preſent. The ſame thing happens on placing a little of the ſame preparation on an electric pile, with the interpoſition of a bit of paper, and drawing a ſpark from its centre by means of a ſlip of metal. The paper will be either pierced or torn.

placed on a lighted coal, or having a ſpark drawn through it from the electric pile.

IX.

Of the Application of Platina on Porcelain. By KLAPROTH.*

Platina never yet applied on porcelain.

PLATINA, as far as I know, has never yet been employed in encauſtic painting. Accordingly I thought it not amiſs, to make ſome experiments on the ſubject, the reſult of which did not deceive my expectations.

Gold and ſilver only have been uſed in this way in the metallic ſtate: and gold anſwers completely, but ſilver does not cover the ground ſo well, and is liable to tarniſh.

Gold and ſilver are the only metals, that have hitherto been employed in the metallic form in painting or ornamenting porcelain, glaſs, or enamel. Gold anſwers this purpoſe ſo completely, that it leaves nothing to be deſired: but ſilver is far from affording equal ſatisfaction. As it is of leſs denſity than gold, it cannot be uſed ſo thin, and does not cover the porcelain or other ſubſtance ſo well. Another reaſon why it is leſs applicable to the purpoſe of painting on porcelain is its property of having its metallic luſtre tarniſhed by ſulphurous emanations, all kinds of which blacken it. This unfavourable circumſtance prevents ſilver from being employed in fine enamel painting, and confines the uſe of the metallic ſubſtances in that only to gold.

* Abridged from Scherer's *Journal der Chimie*, 1802. No. 4. p. 413.

Platina in its qualities ranks with gold, and by its whiteneſs ſupplies the place of ſilver, without labouring under its defects. It not only covers the ground well by its denſity, which even ſurpaſſes that of gold; but like this it reſiſts all the alternations of the atmoſphere, and is not any way tarniſhed by ſulphurous effluvia.

Platina imitates the whiteneſs of ſilver, without having its defects.

The proceſs for applying it is very ſimple. Platina is diſſolved in nitro-muriatic acid, and precipitated by a ſolution of muriate of ammonia. The red cryſtalline precipitate formed is to be dried, reduced to a fine powder, and made ſlightly red hot in a glaſs retort. The muriate of ammonia, which had precipitated in combination with the platina, ſublimes; and the metal remains at the bottom of the retort, in the form of a light, gray powder. This powder being mixed with a ſmall proportion of flux, as is done with gold, and ground with oil of ſpike, is to be applied to the porcelain, put into the furnace, and afterwards burniſhed.

Proceſs of applying it.

Platina applied on porcelain in this manner, is of a ſilver white, ſlightly tending to the gray of ſteel. By alloying this metal in different proportions with gold, different ſhades of this colour are obtained. Platina admits a conſiderable quantity of gold, before its colour undergoes any perceptible change to yellow. For example, if one part of platina be alloyed with four parts of gold, the preſence of the latter cannot be perceived, and the colour ſcarcely differs from that of pure platina. The colour of the gold does not predominate, unleſs it be in the proportion of eight to one.

It reſembles ſilver with a ſlight ſteel tinge. Gold in different proportions varies its ſhade; but does not render it perceptibly yellow, unleſs 4 parts of gold be added to 1 of platina; and the gold colour does not predominate, unleſs it be in the proportion of 8 to 1.

The alloys of platina with ſilver give only a dull metal.

Platina and ſilver give a dull alloy.

Beſide this method of applying platina to porcelain, it may be laid on in the ſtate of ſolution. In this way its colour, luſtre, and appearance, are very different. If the nitro-muriatic ſolution of platina be evaporated to a certain conſiſtence, and laid on the porcelain ſeveral times, the metal penetrates into its ſubſtance, which, after it comes out of the furnace, exhibits a metallic mirror of the colour and brilliancy of poliſhed ſteel.

Platina may likewiſe be applied in ſolution; and this, if ſeveral coats be laid on, penetrates the ſubſtance, and produces a mirror reſembling poliſhed ſteel.

X. *Abſtract*

X.

Abſtract of a Memoir on Galvaniſm, ſent to the National Inſtitute by Mr. RITTER, *of Jena.*†

Erman diſcovered, that an imperfect conductor would take the electricity from an inſulated pile, and become charged like it, one end +, the other —.

TO form a juſt idea of theſe reſearches, it is neceſſary to recal to mind a diſcovery made by Mr. Erman of Berlin, about two years ago, and ſince repeated by Volta before the galvanic committee of the Inſtitute.

If an electric pile, the ſuperior pole of which is poſitive, and the inferior negative, be inſulated, and a communication is made between theſe two poles, by means of an imperfect conductor, as for inſtance, a ſlip of paper wetted with pure water would be for ſuch ſmall quantities of electricity; each moiety of this ſlip would take the electricity of the pole with which it communicated; the upper part would be poſitive, the lower negative.

If this be removed by a glaſs rod, its equilibrium will not be reſtored at once;

but gradually.

Now let us conceive this imperfect conductor to be removed by an inſulating ſubſtance, as a glaſs rod: the equilibrium between the two extremities will not be reſtored inſtantaneouſly, but they will remain poſitive and negative for ſome time, as when they communicated with the two poles of the pile.

This difference will gradually diminiſh in proportion as the oppoſite electricities recombine, and their action, being neutralized, will ſoon become altogether imperceptible.

For this Mr. Ritter ſubſtitutes a pile of copper and wet paſteboard in alternate pieces.

The fundamental experiment of Mr. Ritter is preciſely reducible to this; he merely ſubſtitutes for the ſlip of paper, a pile compoſed of diſks of copper and wet paſteboard. This pile, incapable itſelf of ſetting electricity in motion, at leaſt if we ſuppoſe all its component parts homogeneal, is charged by communication with the pile, like the ſlip of wet paper abovementioned.

Weak electricity, like light, does not paſs freely from one ſurface to another. Hence Mr. Ritter's pile remains charged for ſome time;

But there is an eſſential difference in the reſults. It appears that electricity, when weak, experiences, like light, a kind of difficulty in paſſing from one ſurface to another: at leaſt this ſeems to follow from the experiments of Mr. Ritter, as he himſelf obſerves. The electricity introduced into the pile made with one metal alone, experiences therefore ſome reſiſtance in paſſing from the metal to the paſteboard; and this obſtacle in-

* Bulletin des Sciences, No, 79, p. 145. October, 1803.

creaſes,

creaſes, in proportion as the alternations are more numerous. Accordingly, this pile, when once charged, muſt loſe its electricity very ſlowly, if there be no communication between its poles.

unleſs a communication be made between its poles, when it will give a ſhock, like the Leyden phial.

But if a communication between its two poles be eſtabliſhed, by means of a good conductor, the current of the two electricities and their combination, taking place ſwiftly, will produce a diſcharge, which will act, as in the Leyden phial, by an inſtantaneous ſhock. To this effect a new ſtate of equilibrium will ſucceed, in which the intenſities of the different plates will be diminiſhed in proportion to the quantity of electricity that is inſtantaneouſly neutralized. Theſe diſcharges muſt therefore be reiterated; but growing feebler each time, as the contacts are repeated: but they will ſoon ceaſe to be perceptible, in conſequence of the general equilibrium they tend to reſtore between all the parts of the apparatus; in a word, the action of this pile depends on its becoming alternately a better and worſe conductor, according as the two extremities are made to communicate with each other or not.

Thus the intenſities of the plates will be diminiſhed; till by repeated diſcharges, each weaker and weaker, the equilibrium is reſtored. Its action explained.

The electricity is greateſt at the extremities, nothing in the centre, if inſulated; or greateſt at the top, and nothing at the bottom, if it communicates with the ground.

As to the mode in which the electricity diſpoſes itſelf, it muſt be ſuch, that the repulſive power, or intenſity of each plate, combined with the reſiſtance of the ſurfaces, muſt counterbalance the united actions of all the reſt. In conſequence, if the plates of metal be an uneven number, and the whole apparatus inſulated, the intenſities will diminiſh uniformly from the two extremities, where they will be equal and contrary, as in the primitive pile, to the centre, where they will be null: but if the baſe of the pile have a communication with the ground, the intenſities will increaſe throughout the whole of the pile, from the bottom, where they will be null, to the top, where they will equal that of the primitive pile.

This ſecondary pile produces all the effects of the common pile, but weaker.

The apparatus juſt deſcribed, which Mr. Ritter calls a ſecondary pile, produces ſhocks, the decompoſition of water, and all the other phyſiological or chemical effects obtained from the common pile, but with leſs intenſity. By varying the number and order of the diſks of paſteboard and copper, Mr. Ritter obtained ſeveral intereſting reſults. Thus he obſerved, that of all the modes, in which a certain number of heterogeneous conductors could be diſpoſed, the arrangement that has leaſt alternation is moſt favourable to the propagation of electricity. For inſtance, if a pile be conſtructed of ſixty-four

The fewer the alternations, the more ſpeedily the electricity flows off.

plates of copper, and ſixty-four pieces of wet paſteboard, diſpoſed in three parcels, all the paſteboards being placed together in the middle, and thirty-two plates of the metal at each end, this pile will conduct the electricity from Volta's very freely, and conſequently charge itſelf very little. If the ſeries of wet conductors be interrupted by a ſingle plate of copper placed in the midſt of them, the conducting power will diminiſh; more frequent interruptions will enfeeble it ſtill more; and thus by increaſing the number of interruptions we arrive at a ſeries, in which the conducting power will be ſcarcely perceptible. Theſe phenomena led Mr. Ritter to the knowledge of the reſiſtance, which a weak electricity experiences in paſſing from one ſurface to another: and this reſiſtance takes place only in a weak ſtate, for by a ſingular property, an electricity ſtrong enough to overcome it opens itſelf a free paſſage, and flows off completely.

and the contrary.

Though weak electricity experiences reſiſtance in paſſing from one ſurface to another; electricity ſtrong enough to overcome this paſſes off at once.

We have juſt ſeen, that the conducting power of an apparatus might be varied at pleaſure, by altering its arrangement. It was natural to ſuppoſe, that theſe modifications would influence the chemical and phyſiological effects of the ſecondary pile; and Mr. Ritter propoſed to himſelf, to aſcertain the differences of this influence.

As the arrangement affected the conducting power, it might be ſuppoſed it would influence the effects.

Accordingly he ſought how to divide a given number of wet and ſolid conductors ſo as to form a ſecondary pile, that ſhould receive the greateſt poſſible charge from a given electrical pile. Purſuing this inquiry, he diſcovered two different arrangements, one of which gave a maximum of chemical effect, the other of phyſiological. The firſt conſiſts in a ſmall number of alternations. In this caſe the conducting power is very great, the flow of the fluid continual, and the phyſiological effect but weak. The ſecond, on the contrary, conſiſts in more numerous alternations; in which the conducting power is much leſs, and takes place only at intervals, in momentary diſcharges, when the reſiſtance of the ſurfaces is overcome. In this the electricity eſcapes as it were by jirks, and the chemical effect reſulting from it is ſcarcely perceptible. Theſe differences appear to us to indicate, that the chemical effects depend particularly on a briſk continuous current of the fluid, while the phyſiological require ſucceſſive ſudden diſcharges, entering the organs as if by ſhocks.

And it appeared, that the fewer the alternations, the greater the chemical effect;

and on the contrary, the more numerous the alternations, the greater the phyſiological effect.

The former therefore is produced by a briſk continued flow of the electricity; the latter by repeated ſhocks,

From

From theſe principles we ſee, why the apparatus of Mr. Ritter is better adapted to ſeparate theſe two kinds of action than any other. In the common pile the intenſity increaſes with the number of pairs of metal, and balances the reſiſtance ariſing from theſe alternations; while in the ſecondary pile the intenſity of the two extremities can never ſurpaſs that of the primitive pile, and the reſiſtance produced by the alternations is entirely employed in modifying the current of a given quantity of electricity.

Ritter's pile well adapted to ſeparate the two actions.

In fine, if Volta's pile can thus change the ſecondary pile of Ritter, this property is owing to its intenſity being extremely weak, and as we may ſay imperceptible. A more powerful electricity, ſuch for example as that of the common electrical machine, would paſs through the apparatus completely and not produce the ſame effects.

But Volta's pile could not charge Ritter's, if its electricity were not very weak.

Though theſe deductions appear to us very natural, we offer them only with extreme reſerve, and becauſe we think they agree very well with the facts obſerved by Mr. Ritter.

Theſe deductions not advanced as certainties.

The differences that exiſt in the chemical action of common piles, in proportion to the ſize of the plates, occur likewiſe in the ſecondary piles. The arrangement of the pieces of paſteboard, their thickneſs, the nature of the ſolution with which they are wetted, and the order in which they are interpoſed, as well as various other trifling circumſtances, modify the effects in a thouſand ways, which it would be both uſeful and curious to inveſtigate.

Cauſes of difference in the ſecondary pile.

It would be uſeful and curious to inveſtigate its effects.

XI.

On Spectacles; particularly the Periſcopic. In a Letter from Mr. E. Walker.

To Mr. NICHOLSON.

Lynn, March 19, 1804.

SIR,

ALTHOUGH the improvements which have been made in optics within the laſt century, have been ſuch as even to exceed the expectations of a Newton, yet our ſpectacles, the moſt uſeful of all optical inſtruments, have remained for ages in the ſame ſtate of imperfection. Many alterations, it is true,

Spectacles have not been improved as much as other optical inſtruments.

have taken place in their form, but without producing any better optical effect; and this may occaſion a ſuſpicion, that the properties of the glaſſes made uſe of, have not been clearly underſtood.

General poſition that the rays ought to fall perpendicularly on the lens.

Emerſon obſerves, that "when glaſſes are put into frames for ſpectacles, theſe frames ought not to be ſtraight, ſo as both glaſſes may lie in the ſame plane; but they ought to be ſo bent in the middle, that the axis of both glaſſes may be directed to one point, at ſuch a diſtance as you generally look with ſpectacles. By this means the rays will fall perpendicularly upon both glaſſes, and make the object appear diſtinct. But if they fall obliquely upon the glaſſes, it will cauſe a confuſed appearance in the object *." Martin alſo bent the frames of his viſual glaſſes, for the ſame purpoſe as above deſcribed. And it appears from Dr. Wollaſton's deſcription of his *Periſcopic Spectacles*, that they are alſo conſtructed on a principle to avoid oblique rays †.

Aſſertion: that the perpendicular rays are leaſt adapted to diſtinct viſion.

It is very ſingular, that this mode of conſtructing ſpectacles ſhould have been recommended by ſo many writers on optics, when it is eaſily proved, that the rays of light which fall perpendicularly upon the centre of a lens, enter the eye placed near it, in a more confuſed ſtate than thoſe rays which fall upon a lens obliquely.

General obs. of Dr. Smith.

Dr. Smith, in his complete ſyſtem of optics, gives us this general obſervation on viſion. "That the apparent diſtinctneſs and confuſion of an object depends upon the mutual inclination of the rays to each other in any one pencil when they fall upon the eye." ‡

Illuſtration; by which it is maintained that a pencil of rays paſſing near the axis of a lens will afford viſion leſs diſtinct

This obſervation being admitted, let L L, *Fig.* 1. repreſent a double convex lens, *a o*, a ray of light paſſing perpendicularly through its centre; *c n*, and *d n*, two other rays nearly parallel to the former. The ray *a o* ſuffers no refraction; but the ray *c n* by paſſing through the lens, will be refracted towards the right hand, and the ray *d n*, by refraction, will be turned towards the left; conſequently theſe rays have not a mutual inclination to each other when they fall upon the eye placed near the lens, as at *x*.

* Emerſon's Optics, page 167.

† Philoſophical Journal, V. VII. p. 143.

‡ Elementary parts of Smith's Optics, page 76.

Fig 1.

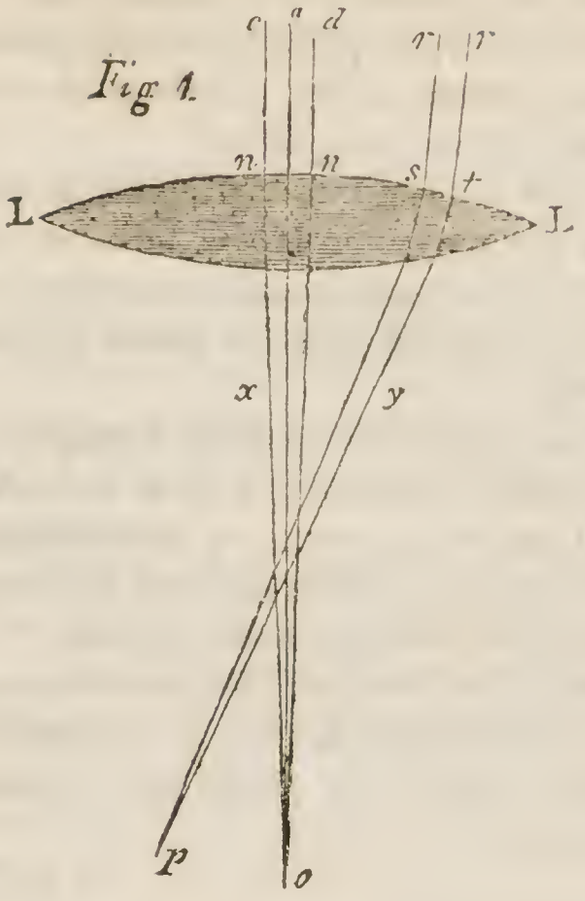


Fig 2.

But, let *r s*, *r t*, repreſent two other rays falling upon the lens in an oblique direction; theſe rays after paſſing through the lens, will both be refracted towards the left hand, and will therefore have "a mutual inclination" to each other when they fall upon the eye at *y*, turned to view an object in the direction *r y*; hence an object viewed in that direction, will be ſeen more diſtinctly, than if it were viewed through the centre of the lens.

—than a pencil paſſing farther from the center.

The ſame thing may be proved experimentally, thus; let a perſon when reading with ſpectacles that are proper for his ſight, look attentively for a moment, at a ſingle word through the centres of his glaſſes. Then let him turn his head, either to the right or to the left, (without moving the book) ſo that the ſame word may be ſeen by oblique rays, and it will appear much more diſtinctly than before.

Experiment offered in ſupport of this doctrine.

From what has been advanced it appears, that when glaſſes are put into frames for ſpectacles, thoſe glaſſes ſhould ſtand in an oblique direction, that the rays of light iſſuing from an object,

Inference: That ſpectacle glaſſes ought to be fixed obliquely,

ject, may not fall perpendicularly upon them. Bending the frames will not effect this purpose, for the glasses must be parallel, as represented in *Fig.* 2. where A B represents the frame, *c c* and *d d*, the two glasses.

I have a pair of spectacles constructed in this manner, but so contrived, that the glasses may be set to any angle, to show the different effects of oblique and perpendicular rays.

The theory of Dr. Wollaston's patent spectacles, may be thus examined.

and that the meniscus is not superior to the double convex.

Let the lens L L be taken away from *Fig.* 1. and a meniscus of the same diameter and focal distance be drawn in its place. Then the rays *r s*, *r t*, after passing through this lens will meet at *(p)* as before, because this lens is of the same focal distance as the double convex, *by Hyp.*

Consequently these rays will fall upon the eye at *y*, in the same state of convergency as before. Whence a lens of the meniscus form, possesses no properties, *in theory*, superior to the double convex.

E. WALKER.

XII.

Communication respecting the Chay Root, a Species of Madder used for dying durable red Colours in the East Indies. By JOHN STOCKWELL, *Esq.* *

Introductory letter to Mr. Taylor.

MR. STOCKWELL presents his compliments to Mr. Taylor, with a sample of an article called Chaya Root, for the inspection of the Society for the Encouragement of Arts, &c. It is in much use on the Coromandel coast, for dying. He accompanies it with a short extract, which will afford some information concerning it.

If Mr. Taylor should cause any experiments to be made upon it, Mr. Stockwell will be obliged by being favoured with the result; and if any person should think favourably of it, and be desirous of a quantity, no doubt it may be had.

East-India Warehouses,
Crutched Friars, 23d May, 1803.

* Made to the Society of Arts, Adelphi, and printed in Volume XXI. of their Transactions.

Extract from the Bengal Commercial Conſultations, the 24th of April, 1798.

To W. A. Edmonstone, Eſq. Secretary to the Board of Trade.

SIR,

I have the honour to acknowledge the receipt of your letter of the 31ſt ult. informing me that I was deſired by the Board to make ſome experiments with the Chay Root, and to report whether I think it will be of any uſe to the dyers or calico-printers at home. Letter to the Bengal Board of Trade.

It is rather ſurpriſing, conſidering the pains that have been taken in Europe to diſcover, or at leaſt to imitate the method of dying the *Turkiſh* or *Adrianople* red, that ſo little attention has been paid to the equally beautiful and permanent red given to their calicoes by the natives of the coaſt of Coromandel. Although full accounts of the practice of calico printing in the Eaſt Indies were ſent home long ago by the miſſionary Cœur Doux, M. Poivre, and others, it does not appear that the European artiſts have ever tried their ſkill in the *Chay* root, the drug by which the admired red colour is produced. I have never heard, at leaſt, of any ſuch attempts, nor do I believe that the root has ever been ſent home. It is evident, from the manner in which this drug is mentioned by Dr. Bancroft, in his "Experimental Reſearches," p. 174, that well-informed writer had never ſeen it, which I think could ſcarcely have happened, had it been at all known to the London dyers. The Chay-root affords a beautiful and permanent red to cottons. Not practically known in Europe.

It is probable that the tedioufneſs of the Indian proceſs, as deſcribed by thoſe who ſent home the accounts of it, conſiſting of many tireſome manipulations, continued during a period of nineteen or twenty days, deterred the European artiſts from trying the effects of the *Chay* root in dying or printing their cottons. What may appear more extraordinary is, that the ſame cauſe, co-operating perhaps with the natural indolence of the people, and their having cheaper though inferior red dyes at hand, has prevented the uſe of this root from obtaining in Bengal; for, ſo far as I can learn, it is not uſed in this part of India. The natives here are content with the red produced by the *Aul Munjiet* and other drugs, though the colours yielded by theſe are far inferior to thoſe of Madras calicoes. The Indian proceſs apparently tedious. Cheaper drugs uſed at Bengal.

On

On this account I am ſurpriſed at the meaſure which has been adopted, of ſending round hither ſuch a large quantity of the *Chay root* from the coaſt. I apprehend very little or none of it will be purchaſed at the approaching ſale, in which caſe it muſt be either returned to Madras or ſent to England. The expediency of this laſt meaſure muſt depend on the poſſibility of ſo abridging the proceſs of dying, or printing cottons with *Chay root*, that the Engliſh artiſts may find it worth their while to have recourſe to it for the reds and purples, inſtead of the *Smyrna madder*, or whatever other drugs they uſe for thoſe purpoſes at preſent.

Experiments to ſhew whether the Chay root might be of value in Europe.

To aſcertain this point, I have, agreeably to the inſtructions of the Board, made ſeveral experiments with the *Chay root;* indeed as many as the ſhortneſs of the time, and my other avocations would permit. From the reſult of theſe trials, I entertain great hopes that the Engliſh calico-printers will not only ſhorten the proceſs, ſo much as to finiſh it within a tenth part of the time required for the Indian proceſs, but that they will by the *Chay root* dye their cottons of a brighter red than can be done by madder, or any other vegetable.

Cotton was *burred* or galled;

It is needleſs to detail the many trials I made, which either failed altogether, or ſucceeded in a very imperfect degree. I ſhall therefore only mention that proceſs which I found to anſwer beſt, and by which the piece of cotton cloth, which I herewith ſend you for the inſpection of the Board, was printed. Having made a decoction of two ounces of powdered *Hurr*, (the fruit of the Myrobolona Citrona*) in a quart of water, I took a piece of Madras cotton cloth, and boiled it in the de-

then waſhed;

coction for about half an hour. Having taken out the cloth, and waſhed it well with cold water, I dried it in the ſun, and afterwards had it properly ironed and ſmoothed for the pencil.

and a deſign made with acetite of alumine;

I then took ſome of the acetite of alumine, made in the manner directed by Dr. Bancroft, and, thickening it properly with gum-arabic, I delineated a flower with this mordant upon the

waſhed,

cloth, and dried it in the ſun. I afterwards waſhed the cloth in cold water, to clear it of the ſuperfluous acetite, and dried it again in the ſun. I then infuſed about two ounces of the *Chay root*, coarſely powdered, with about a quart of water, in a veſſel well tinned, and ſetting it on the fire, as ſoon as the

* Aleppo galls will probably anſwer as well as the Hurr.

liquor

liquor began to grow warm, I put the cloth into it, and let it remain until it had boiled about half an hour, during which the cloth was frequently ſtirred. I then took it out, and having rinced it well with cold water, I put it to dry in the ſun. The delineation of the flower now appeared of a good bright red, and the ground of the cloth, though ſlightly tinged, was much leſs ſo than I expected. By waſhing it again with cold water, and afterwards with ſoap and water, and expoſing it for a whole day to the ſun, during which it was frequently beſprinkled with water, I brought it to the ſtate in which you now ſee it. The whole proceſs has only taken up about ſix and thirty hours. I ſhould have mentioned that the waſhing with ſoap heightened the brightneſs of the red conſiderably.

and boiled in the infuſion of Chay-root.

It gave a good red;

which ſtood waſhing, was improved by ſoap, and cleared by expoſure to the ſun.

Making allowance for my want of experience in the practice of dying, and conſidering the great improvement which may be expected in the proceſs from the ſuperior ſkill of the Engliſh artiſts, I think we may conclude, even from this imperfect eſſay, that the *Chay root* will be a valuable acquiſition to the Engliſh calico-printers. I therefore recommend, that ſo much of the root as may remain unpurchaſed at the ſale, or at leaſt that a part of it may be properly packed up and ſent to England by one of the ſhips now under diſpatch.

This rough eſſay is encouraging.

This drug is the root of a plant called by the botaniſts *Oldenlandia Umbellata*. I have not met with it in this part of India; but it grows naturally on the coaſt of Coromandel, where it is alſo cultivated in great abundance, for the uſe of the dyers and calico-printers.

Some account of the drug.

The ſample which you ſent me appears to be of a good quality, and in good condition. It is ſaid that the root will remain with its virtues entire for ſeveral years, and that they are even improved by keeping. If this be the caſe, and provided the dyers at home find it anſwer, this circumſtance is a very favourable one, and muſt enhance the value of the drug as an article of commerce.

I am, &c.

(Signed) J. FLEMING,
Inſpector of Drugs.

Export Warehouſe,
6th April, 1798.

Minute

Minute of the Board of Trade, dated April 10, 1798.

Opinion of the Board of Trade, that the root will be acceptable to calico-printers, and of value to the E. I. Company.

THE Board are of opinion, that the reſult of the Inſpector's experiments will afford very acceptable information to the calico-printers in England; and, if their operations ſhould prove his ideas to be well founded, the plant may prove a valuable acquiſition to the manufacturers of Great Britain, and alſo an article of commerce from the coaſt particularly uſeful, as there is a want of coaſt articles of low value, as well light, as a ſufficiency of ponderous, to make up a proper cargo for a large ſhip, without ſwelling its value to too great a riſk, as would be the caſe were a ſhip to be loaded entirely with piece goods.

XIII.

Deſcription of an Apparatus for raiſing Water by the Fall of Waſte Water. By Mr. SHARPLES, *Portrait Painter, Bath. Communicated by the Author.*

Advantages to be derived from the fall of water.

EVERY drop of water that falls may be conſidered as a natural power capable of doing ſome work; and it would be productive of conſiderable benefit in many domeſtic and manufacturing exigencies, if ſmall ſtreams and currents deſcending along the ſides of hills, or even through the meadows, were employed, by the application of ſimple machinery, to aſſiſt the labours of man. I am informed that numerous little mills for grinding corn are diſtributed over the face of the country, in Portugal, upon very inconſiderable rivulets; where they not only add to the general product of human work, but may alſo perhaps tend to equalize the price of grain to the conſumer. And indeed if it be conſidered that a ſmall ſtream of about a yard wide, running over an obſtacle to the depth of an inch, and having ſpace to deſcend through twelve inches, would do half as much work in the 24 hours as a horſe, whether in grinding corn or turning an engine, or raiſing water for uſeful purpoſes, we ſhall be ſtruck with the neceſſity of enquiring whether any falling water whatever ſhould be ſuffered to deſcend without due employment in its courſe.

Small mills in Portugal.

A very ſmall brook may do work.

The

Engine for employing waſte water.

The ingenious gentleman who has favoured me with the ſketch from which Plate XVI. was made, has directed his invention to teach us how the waſte water of our apartments, and other ſimilar places, may be made to raiſe other water in its deſcent; and the ſimple engine he propoſes for that purpoſe conſiſts of the following parts:

Deſcription. It is a ſyphon with compartments.

A, B, and C repreſent three veſſels or compartments. The upper compartment A is an open trough, or waſte receptacle, which can be emptied into the ſecond cloſe veſſel B, by turning the cock D; which cock, by the ſame motion, opens the communication-pipe I and an air-pipe H, to ſuffer the included air to iſſue out. N and M are the two legs of a ſyphon; the ſhorter N, communicating with the reſervoir O, containing water, and the longer M, communicating with a trough P, at a lower level which overflows into the drain. The cock F, by the ſame turn, opens or ſhuts both theſe pipes at once. And laſtly, the pipe G forms a communication from the upper part of the receptacle B to the upper part of C. The proceſs may be thus explained:

The foul water runs down one leg, and clean water aſcends by the other.

When D is opened, the middle veſſel B receives waſte water from A. If D be then ſhut and F opened, the longer ſyphon-leg M admits the waſte water to flow into P; leaving a ſpace in B wherein the air in C as well as B expands (with a diminiſhed ſpring) by means of the pipe G. The preſſure of the atmoſphere on the water in O will therefore raiſe that fluid into C, provided the height be not too great; and if the difference between the heights of the veſſels B and C be equal to that between O and P, the quantity of freſh water raiſed may equal that of the waſte which deſcends from the greater height.

The cock E ſerves to draw off the water ſo raiſed; at which time it is convenient that the cock F ſhould be ſhut; and this ſhould alſo be done whenever the cock D is opened. This may eaſily be made to take place by a ſimple connection between the cocks, without depending on the ſkill or diligence of the ſervant.

W. N.

XIV.

On Gridiron Pendulum Rods compoſed of Lead and Iron. By Mr. BENZENBERG *.

Mr. B. employs lead, on account of its great dilatability.

MR. B. was induced to employ lead on account of its great dilatability, which is to iron as 2.57 to 1, ſo that 16.5 inches of lead compenſate 1.3 of iron; and he choſe iron in preference to ſteel, becauſe eaſier to work. The compenſation was made by a ſingle rod in the centre, $16\frac{1}{2}$ inches long, French meaſure, and $\frac{1}{2}$ an inch thick. It was ſimply pinned into gorges in the croſs-piece of copper; but the other parts of the gridiron were rivetted in the uſual way. The iron rods were made of the beſt thick iron wire.

His compenſation made by a central rod, $16\frac{1}{2}$ inches by $\frac{1}{2}$ an inch.

Its advantages.

The materials of this pendulum are cheap, and it may be made in a couple of days. As the preſſure takes place in a vertical direction, there is no danger, according to Mr. B. of rods of theſe dimenſions bending.

Mode of correcting the compenſation.

To correct the compenſation, the central rod of lead muſt be left ſo long that we may be ſure the compenſation is in exceſs. The quantity of error may then be found by the freezing apparatus, and how much it is requiſite to cut from the rod may be calculated with the greateſt exactneſs.

XV.

On the Advantages of a large Knife in making Pens. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

The edge of a large knife is better than that of a ſmall one.

USEFUL diſcoveries are ſometimes derived from the common occurrences of life; and that which I am now going to mention, is a mere accident of this kind. About three years ago, I took up a large pocket knife which had been newly ground, and uſed it for making a pen, and was not a little ſurprized to find that it anſwered the purpoſe much better than

* *Voigt's Magazin fuer den neveſten Zuſtande de Naturkunde*, Vol. IV. p. 787.

any

ny penknife I ever had. I mentioned this circumſtance ſoon after to the cutler who made the knife. He ſaid that he had no doubt of the truth of my obſervation, as he could harden and temper a large blade better than a ſmall one; becauſe there is more difficulty in aſcertaining the proper degree of heat in the latter than in the former.

And it may perhaps be a mechanical truth, that a ſtrong knife will overcome the reſiſtance of a hard quill with leſs exertion of the hand than a weak one. The blade of the knife that I uſe, and which gave riſe to theſe obſervations, is four inches in length and half an inch in breadth. And perhaps the mechanical effect.

It may perhaps be ſuppoſed that a knife of theſe dimenſions is improperly conſtructed for cutting the ſides of a pen into a curve, but it will be found, on trial, that the breadth of the blade is no impediment, for it is the form of its edge which it receives by whetting, that renders a broad blade quite as manageable as a narrow one. It may with convenience and advantage be uſed as a penknife.

I am, SIR,

A conſtant Reader of your

Truly valuable Journal,

March 22, 1804. W.

XVI.

Communication reſpecting an Article ſent from the Eaſt Indies, under the Name of Gum Kuteera, and of which a large Quantity has been lately imported into this Kingdom. By Mr. JOHN COWLE *.

To Mr. TAYLOR.

SIR,

THERE is a gum produced in ſeveral parts of Oude and the adjacent provinces, ſo nearly reſembling gum tragacanth, as to have been taken for it by many; and large quantities of it, of late years, have been imported into Europe, under this miſtaken opinion. But it is now well aſcertained, that this gum (which in the country language is called kuteera) is the produce of a particular tree, of quite a different ſpecies from the Gum reſembling g. tragacanth,

* Society of Arts, Vol. XXI.

thorny

thorny buſh which yields the tragacanth; and being imperfectly ſoluble, and poſſeſſing but little of a glutinous nature, renders it inapplicable to the purpoſes for which gum tragacanth is uſed. I am nevertheleſs of opinion, that this gum might be found ſerviceable, in one way or other, to ſome of the arts and manufactures, and am therefore induced ſhortly to deſcribe it, and the uſe to which it is applied in India, with a view of exciting any of your ingenious correſpondents to make experiments on it, and determine its real value, or abſolute inutility.

but not ſo glutinous, or applicable to the ſame uſes.

Deſcription of gum kuteera.

The kuteera is in looſe wrinkled drops or pieces, void of ſmell and taſte, of a whitiſh colour, and moſtly tranſparent. In water, it ſlowly forms itſelf to a pulp or jelly, and is nearly taſteleſs; but, if pounded well in a mortar, and then boiled in water for fifteen minutes, ſtirring it all the time, it will be found *completely diſſolved*. A tea-ſpoonful of this powder gives to three pints of water the conſiſtence of capillaire. Might it not in this ſtate be of ſervice to painters and artificial floriſts, or even uſed to give a gloſs to ſilks? The natives of India make a varniſh by mixing kuteera with other gums; and I have been told, they likewiſe make uſe of it in the printing of calicoes. It is the principal ingredient in a medicine they give to their horſes in certain diſeaſes, and in this reſpect is of very general conſumption.

It promiſes to be uſeful in other reſpects.

Large quantities are now in the India warehouſes.

I ſhall only add, that many tons of this gum are now lying in the Eaſt India Company's warehouſes, totally unſaleable, or that will not ſell for more than the firſt coſt in India; and if this notice ſhall be attended with beneficial conſequences, I ſhall derive great ſatisfaction in the reflection of having ſerved the intereſt of the ſpeculators in this drug, amongſt whom I could formerly number myſelf; and I believe I was the firſt importer of it into this country.

I am, SIR,

Your moſt obedient Servant,

JOHN COWIE.

Finſbury-ſquare,
25th May, 1802.

XVII.

Galvanic Experiments with Ice, and a Method of rendering the Electric Attraction of the Pile very evident. By S. P. Bouvier, *of Jodoigne, Member of the Society of Emulation, of Natural History and Physics, at Brussels* *.

I AVAILED myself of the first frosts this year to make some experiments on ice, both as an interposed substance, and as an exciter and conductor.

Experiment 1. A pile was formed of eighty pairs of plates of zinc and silver, and as many very thin disks of ice. It not only did not give the slightest shock, but did not excite the least taste on the tongue, or produce the smallest luminous effect. I left it standing for several hours, but found no difference in the result.

A pile of 80 pairs, separated by thin disks of ice, produced no effect.

Exp. 2. I arranged ninety disks of ice in pairs with as many crown pieces, and placed between the pairs pieces of pasteboard impregnated with a solution of salt. This combination produced no more effect than the preceding.

A pile of 90 pairs of ice and silver, with wet pasteboard interposed, had no effect.

Exp. 3. The same number of disks of ice were paired with as many of zinc, and wet pasteboards interposed as before, with the same result.

Ice with zinc was equally inactive.

Exp. 4. I made a pile of a hundred and twenty-eight pairs of zinc and silver, interposing pasteboard impregnated with a saline solution. This pile gave such strong shocks, that they extended to the shoulders with violence. On forming the circle of this pile with the hands armed with little pieces of ice, all the power of giving a shock was intercepted. I then took a piece of ice in my mouth, and brought it into contact with the summit of the pile, while with my hand I touched its base; but no taste was perceived on my tongue.

128 pairs of zinc and silver, separated by wet pasteboard, gave strong shocks felt in the shoulders; but if touched with a bit of ice held in the hand, nothing was felt. It could not even excite taste, when the communication was made with ice.

The thaw prevented my pursuing this inquiry any farther. If the frost return, I will pair disks of silver with cast disks of muriate of lime, of pure potash, and of sulphur of potash, in order to produce in the first two cases an action of physical solution, and in the last a chemical action, after the manner of Davy.

Farther experiments proposed with muriate of lime, pure potash, and sulphuret of potash.

* Journal de Chimie, par Van Mons, for January 1803, No. XI. p. 52.

Exp.

On a pile of 140 ſeries, a delicate magnetic needle was ſuſpended; on approaching the hand to it, an oſcillatory motion took place, but the magnetic attraction appeared to be ſtronger than the electric.

Exp. 5. On a pile of 140 ſeries of zinc, ſilver, and paſteboard, impregnated with a ſolution of ſalt, I placed an iron pivot, and on this pivot a very delicate and ſenſible compaſs needle. The friction was almoſt nothing. I firſt applied one hand to the baſe of the pile, and I carried the other near to the needle. I obſerved a ſlow and oſcillating approach; but the needle was inceſſantly invited to reſume the magnetic direction, whence I inferred, that the polar attraction exceeded the electric. I tried then to render the tranſmiſſion of the fluid more eaſy, by making it glide over a metallic conductor, as a braſs wire; but the reſult was the ſame as when I made the fluid paſs through my arm.

A common pin, ſuſpended in the ſame way, was attracted by a wire communicating with the pile, at the diſtance of ſome lines.

Exp. 6. To the magnetic needle uſed in the preceding experiment, I ſubſtituted one made of a pin, the movement of which alſo I rendered very free. I approached this needle with the extremity of a braſs wire that communicated with the baſe of the pile, and I perceived it turn with rapidity, at the diſtance of a few lines, to ruſh againſt the wire. A ſimilar communication eſtabliſhed through the arms afforded the ſame reſult.

A ſilver thread, ſuſpended from a knitting-needle, placed on the ſummit of the pile, was attracted by the finger.

Exp. 7. I placed on the ſummit of the pile an iron knitting-needle, bent, and ſuſpended to it a ſilver thread, as it is called, uſed for embroidery. I completed the circle with one of my fingers, approaching the thread gradually with it. At a certain diſtance the thread applied itſelf to my finger, and remained adhering to it, though it was perfectly dry.

When the communication was made with a braſs wire, vivid ſparks were given out, the ſilver was oxided, and part of it melted. The ſame effects took place repeatedly.

Exp. 8. I repeated the preceding experiment, only forming the communication by means of a braſs wire. The attractive effect equally took place, and very vivid ſparks were given out between the wire and the thread, which oxided the latter over all its ſurface, and melted a part of it ſeveral lines in length.

All theſe experiments were repeated a great number of times, and always with the ſame ſucceſs.

A ſilver thread, ſuſpended from a wire fixed to the ſummit of the pile, was attracted by one hand, when the other was dipped

Exp. 9. In the upper diſk of a pile of 97 pairs of zinc and ſilver, I fixed an iron wire, and ſuſpended to it a ſimilar ſilver thread. At the foot of the pile was a communicating baſin, in which was a ſolution of ſalt. Dipping one hand into the baſin, and bringing the other near the thread, this immediately came and ſtuck to it. When I took my hand out of the baſin, the

the thread immediately quitted the other, but ſtuck to it again when I redipped my hand into the baſin; and ſo on alternately.

into a baſin of ſalt and water communicating with the pile.

Exp. 10. I repeated the preceding experiment, fixing a braſs wire, in order to obtain more firmneſs in the contact with the thread, into a block of lead, placed on the table on one ſide of the pile. I bent the wire into a crook, and ſuſpended from it an embroidering thread, which I placed at a ſmall diſtance from the knob of the pile. As often as I held one of my hands wet againſt the block of lead, and dipped the other into the water in the baſin, the thread applied itſelf to the button of the pile, and ſtuck there; but it quitted it as ſoon as I interrupted the communication by withdrawing either hand. This experiment, repeated more than fifty times following, was conſtantly attended with the ſame reſult.

A ſilver thread, ſuſpended from a wire fixed in a block of lead on the table, was attracted by the knob of the pile, when one hand was dipped into the baſin, and the other, wetted, touched the lead; but on removing either hand, the effect ceaſed.

XVIII.

Experiments on the Activity of a Galvanic Pile, in which thin Strata of Air are ſubſtituted inſtead of the Wet Bodies. By Mr. Dyckhoff *.

Mr. RITTER has obſerved †, that all ſubſtances which, when interpoſed between the conſtituent parts of the pile, excite electricity, are active only ſo far as they contain moiſture; and he aſſerts, that it is impoſſible to conſtruct an active pile without the intervention of a wet ſubſtance. I take the liberty to oppoſe to this aſſertion an experiment, by which it is contradicted. I conſtructed a pile with diſks of copper and zinc, and little bits of thin green glaſs, about the ſize of a lentil, three of which I placed triangularly in the intervals that ſeparated the metallic plates. Thus between each pair of metals I had a thin ſtratum of air inſtead of a wet ſubſtance; and the following were the reſults:

Ritter aſſerts, that moiſture is neceſſary in Volta's pile.

But a pile of copper and zinc in pairs, ſeparated by bits of glaſs, ſo as to interpoſe air alone, exhibits electricity;

1. A pile of ten pairs, tried by the condenſer, affected the electrometer as powerfully as a common pile of five pairs.

though only in a degree equal to that of a common pile half as high.

* *Voigt's Magazin fuer den neueſten Zuſtand der Naturkunde*, Vol. IV. p. 791.

† Intelligenzblatt der allgemeinen Litteraturzeitung, 1802, No. 193.

Damp air did not anſwer ſo well as dry, apparently.

2. The action remained invariable as long as the air continued dry; but damp air appeared leſs favourable to the pile.

The electricity was transferrable to the Leyden phial.

3. The ſame degree of electricity was communicated by the pile to a Leyden phial.

The weakneſs of the electricity perhaps owing to the roughneſs of the plates.

The plates were only three inches in diameter. As they had already been uſed for a pile in which wet ſubſtances were interpoſed, and were much corroded, I took off the oxided portions by a file; but the plates ſtill remained rough and uneven; and to this circumſtance I aſcribe my not obtaining a higher degree of electricity. I have yet had no opportunity of repeating the experiment on a larger ſcale; but the reſult I obtained appeared to me ſufficient to render the aſſertion of Ritter at leaſt queſtionable. I ſee no reaſon why glaſs, thin ſtrata of ſealing wax, or the like, ſhould not have the ſame effect on Volta's pile as in the condenſer. For the experiment to ſucceed, it is neceſſary that the bits of glaſs ſhould be as thin and even as poſſible; for, if the ſtratum of air be too thick, it is natural that the electricity, which is excited in ſuch a feeble degree by the contact of the two metals, ſhould remain inactive on the adjacent metallic pairs.

No reaſon why glaſs, &c. ſhould not act in the pile, as in the condenſer.

The glaſs muſt be as thin and even as poſſible, that the ſtratum of air may be ſo.

XIX.

Extract from a Letter from Mr. J. B. Van den Sandl, *Profeſſor at Luxembourg, on the Efflorescence and Decompoſition of Glaſs Tubes* *.

Oxidation of white glaſs not uncommon.

I HAVE lately had an opportunity of obſerving at leiſure the common phenomenon of the calcination of white glaſs. I expoſed to the flame of an enameller's lamp ſome old barometer tubes that appeared not to have any defect, in order to bend them into ſyphons. Scarcely had they come into contact with the flame, when they turned white and opake. One of the tubes being wetted, I placed it on a chamber-ſtove to dry; and before it was heated, it was covered with a whitiſh efflorescence. Heating the others produced on them the ſame effect. I then removed the efflorescence, replaced the tubes on the ſtove, and they became white afreſh. By continuing the ex-

Old barometer-tubes expoſed to flame, turned white and opake;

and the ſame if ſimply heated.

* Journal de Chimie, par Van Mons, IV. p. 232.

periments,

periments, I reduced an eighth part of the glaſs to a very fine powder; after which the tubes were opake, and had loſt their poliſh.

⅛ of the glaſs thus effervefced.

The greater part of this efflorefcence exhibited in the microſcope rhomboidal parallelograms; the reſt triangles, moſt of which were iſoſceles.

in rhomboidal parallelograms or triangles.

Having poured nitric acid on a portion of the efflorefced matter, it diſſolved 0.20. One part of this ſolution afforded, on evaporation, cryſtals of nitrate of ſoda: the other afforded no precipitate with potaſh.

The efflorefcence was ſoda.

Theſe tubes were become good conductors of the electric fluid; which led me to preſume, that their decompoſition was occaſioned by the humidity of the place where they were kept; which humidity, after it had penetrated the ſurface of the glaſs, imperceptibly inſinuated itſelf into its pores. This efflorefcence, however, depends much on the quality of the glaſs; for we may obſerve in the ſame glaſs frames, with a ſouth or ſouth-weſt expoſure, ſome panes calcined to ſuch a degree as to become entirely opake, while others retain their tranſparency, and are perfectly intact.

Theſe tubes had become good conductors of electricity; the decompoſition therefore owing probably to humidity.

But the efflorefcence depends much on the quality of the glaſs.

XX.

On the Criterions or due Diſcriminations of Cyder Fruit. By THOMAS SKIP DYOT BUCKNALL, *Eſq.**

To CHARLES TAYLOR, Eſq.

SIR,

HEREWITH you receive a paper on the criterions or due diſcriminations, by which the valuable cyder fruit may be aſcertained, referring to my laſt and preceding Papers on Varieties.

On cyder fruit and the making of cyder.

Report ſays, cyders are not well produced out of the cyder countries. Were it ſaid they are not well made, the aſſertion would be juſt; but it is a miſtake to ſuppoſe that cyder cannot be made in perfection out of thoſe ſituations. Only chooſe the proper ſoil for the fruit, ſee it ground down to a perfect ſmoothneſs, rind, pulp, kernels and all; duly fermented.—Tun the liquor at the exact point of time with regard to ſettling, which

* Soc. Arts, Vol. XXI.

On cyder fruit and the making of cyder.

is well underſtood in the Weſt, and not attended to elſewhere; and do not rack it too much. Keep the liquor in quantity to a good age; and I take the liberty of maintaining, that there are places in Hertfordſhire capable of producing as good and fine cyder as any in Herefordſhire.

I was once aſked what are the impediments to a more general fine crop of fruit? My reply was, the coldneſs and uncertainty of the climate, injudicious planting, with inattention and neglect in thoſe who have the management of it. As for planting and guarding againſt cold, enough has been ſaid on that ſubject through the whole of my treatiſes. It has been obſerved the cyders were better one hundred years ago than they are now. That may have been the caſe; and I ſhould account for the difference as follows:—When the gentry and ſuperior yeomanry of the country depended on barley and fruits to make an agreeable beverage for their friends, it is natural to ſuppoſe that they perſonally ſuperintended the buſineſs themſelves, and received great ſatisfaction when their labours were attended with ſucceſs; whereas, when Port wines became the luxury of the country, cyder, of courſe, naturally fell into diſuſe at the better tables. Hence the maſters of families were no longer zealous to produce fine potent liquor, and giving it as cyder, that being aſſigned to inferior uſes.

Undoubtedly, where cyder is the general drink of the country, great quantities muſt be raiſed for that purpoſe; and it is done at ſo low and eaſy a rate, as to be ſent from the mill to the neighbours' cellars under three half-pence the gallon. What I am attempting is, to be able to produce, *at a certainty*, a fine generous liquor, ſuch as may do credit to the name of the cyderiſt, and which will depend much more upon attention than is generally imagined.

After having expatiated on the old and new varieties of the valuable cyder fruits, in the laſt volume of the Society's Tranſactions, I have nothing farther to offer for the purpoſe of cloſing the Orchardiſt, than to preſent my beſt reſpects to the Society, and give them the marked criterions as follow:—

My friend, Thomas Andrew Knight, Eſq. has made good diſcriminations in his Tract on the Apple and Pear. The Somerſet report has further extended the ſubject.

I ſay, chooſe an apple naturally ſmall, of a whitiſh colour, ſomewhat tinged with red; of a fine yellow pulp, with a cer-

tain degree of aſtringency, and as much ſaccharine matter as nature is diſpoſed to produce; then obſerve if the cells are large, and full of ripe kernels; and further ſtill, know whether the bloſſoms are patient of cold, and the fruit ripens well. On cyder fruit and the making of cyder.

Such an apple, particularly if a new variety, properly ground down and duly fermented, muſt make good cyder; and both as to profit and uſe, be valuable in any neighbourhood, or as an article of trade. The foundation is now ſo well laid, in the ſeedling beds in the county of Hereford, that within five years there will be more than one hundred new valuable varieties produced. I laſt autumn ſaw two moſt beautiful new apples, of the firſt year's growth, upon the Grange eſtate, and which decidedly obtained the Hereford premium: they were ſeedlings of high promiſe. There is an emulation among gentlemen of that part of the country, which does them great credit.

Dr. Symonds gives ſeveral of theſe diſcriminations; and in addition ſays:—The flavour of a good cyder-fruit cannot be miſtaken by a man converſant with apples, though difficult to be deſcribed; but above all, he recommends to chooſe apples, the rinds of which have follicles, or cells, containing large quantities of eſſential oil, more particularly to be noticed in the old ſtire, golden pippin, and pauſon, and from which Dr. Symonds conceives the cyder in a great meaſure derives its flavour. It is to be obſerved, that the old pauſon, woodcock, and red muſk, are generally large apples. The old ſcorched-harvey is an exception as to the yellow colour; that being white, with a brown ſkin. There are other exceptions; but the material criterions are here enumerated according to the received opinion.

Had I formed theſe diſtinctions when I was a boy, I ſhould then have ſaid, take any apple bordering on the golden rennet or golden ruſſet, and it will make good cyder, in conſequence of ſuppoſing that the golden colour is of ſervice. We had a fine apple, the royal pearmain; it was more flat and large than the Seville orange; it had a thin ſkin and quick taſte, with much ſweetneſs, and made fine cyder: but I preſume that variety is now over, as I have not ſeen one of them for many years.

In the October of 1801, which I ſpent in Kent I was ſhown a new apple. The tree is handſome, and the fruit has moſt of the

On cyder fruit and the making of cyder.

the criterions, but does not ripen well, which is a great defect in cyder making; we named it *The Bland of Hartlip.* It has the macula, or follicles in the ſkin, more viſible than on the golden pippin. I intend examining it with attention at a future period, as it is not poſſible to form a certain judgment on it for ſome years. My reaſon for mentioning the Bland of Hartlip is, to prove that the idea of ſearching after new varieties has made a general impreſſion throughout the kingdom.

It was the cuſtom formerly, if a new apple had many good points, but ripened late, to preſerve the tree as a new variety, imagining that, as it advanced in age and acquired ſtrength, the fruit would ripen the ſooner, and conſequently the cyder be richer. I ſhould add to it, lay the land dry, and ſpread plenty of manure. Theſe attentions would much accelerate the wiſhed-for object of improving the liquor, as well as increaſing the quantity of fruit.

I remain,

DEAR SIR,

Your obedient Servant,

THO. SKIP DYOT BUCKNALL.

Hampton-Court, Nov. 14, 1802.

XXI.

Concerning the Steam Engine from Mr. Blakey, by a Correſpondent. B.

To Mr. NICHOLSON,

London, March 20, 1804.

SIR,

Introductory letter.

SOME time about the year 1776, I met with an engineer of the name of Blakey, in Holland, of whom Ferguſon the philoſopher and ſeveral other ingenious men have ſpoken in high terms. I do not know whether there is any hiſtory of him or of his works; but as many of his contemporaries muſt ſtill be living, I ſhould hope that this notice may produce ſome information through the medium of your Journal. I have lately met with a pamphlet by this author, intitled "*A ſhort hiſtorical Account of the Invention, Theory, and Practice of Fire Machinery;*" printed at London, in the year 1793, without book-

ſeller's

ſeller's name. It chiefly conſiſts of ſhort notices of the author's own operations, with academical teſtimonies; and I underſtand from private information, that it was publiſhed at that time by Mr. B. in indigent circumſtances, with a view to obtain ſubſcriptions for a large treatiſe, which has not ſince appeared. Some of your correſpondents may probably favour us with an account of the diſpoſal of thoſe writings ſince his deceaſe. I have extracted the outline of hiſtory of the ſteam-engine from the beginning of his pamphlet, which, if you think will be acceptable to your readers, is very much at your ſervice.

I remain, Sir,

Your obliged Reader,

R. B.

IN the beginning of the ſeventeenth century, a modern philoſopher conceived the notion of making uſe of fire and water to obtain motion. This was Brancas of Rome, who contrived a ſtamping-mill to be worked by ſteam, coming from a large æolipile, as may be ſeen in his twenty-fifth plate. We are obliged to him for a number of ingenious inventions, which he dedicated to M. Canci, governor of Loretto, in 1628, and publiſhed his work at Rome the year following.

Brancas firſt applied Steam to mechaniſm; *or propoſed it*, 1627.

Thirty-four years after, the Marquis of Worceſter publiſhed a little treatiſe, under the name of "Century of Inventions, printed in London, 1663;" he gave them as if he had tried them, and propoſed other engines of great utility, which only wanted encouragement from government: "Several were nothing but projects," ſays Deſaguliers, "but, where he is more explicit, there is one for raiſing water by means of fire."

Marquis of Worceſter, 1663.

In the acts of Leipzig, 1690, p. 410. is to be ſeen an invention, intitled, "A new manner to procure a conſiderable quantity of power with little expence, by Dr. Papin." "In this new engine," he ſays, "of which we have given a deſcription in the acts of Leipzig, 1688, and the intent of which was to obtain a new method of making uſe of gunpowder as a power to give motion, the chief fault was, that the gun-powder, in the under part of the tube, did not entirely fill

Papin, 1690,

propoſed a piſton and condenſation.

fill the capacity of this tube, ſo as to have no air under the piſton *." Papin continues to give reaſons like a man of great underſtanding; but as there muſt be plates and deſcriptions to explain the whole, I ſhall ſay no more on the ſubject, but only add, that the doctor did his beſt to obtain his end in another manner. He ſhews that, with "A little water changed into ſteam by means of fire, we can have an elaſtic power like air; but that it totally diſappears when chilled, and changes into water; by which means he perceived, that he could contrive a machine in ſuch a manner, that, with a little fire, he would be able, at a ſmall expence, to have a perfect vacuum, which could not be obtained with gunpowder." Among various contrivances, this appeared to anſwer the beſt. In the work itſelf a plate of this contrivance will be given, which, however, is not to be compared to what has been invented ſince Papin, by men who had never heard of him, and were altogether much inferior to him in theoretical and practical philoſophy.

Capt Savery, 1699.

In 1699, a remarkable year for the honour of Engliſh ingenuity, Captain Savery ſhewed to the Royal Society an engine which raiſed water by means of the expanſion and condenſation of ſteam; he publiſhed a little treatiſe, which he named "The Miners' Friend;" the deſcription of this engine is to be ſeen in Harris's Dictionary of Arts, &c. printed in London, 1704. This learned man ſays of Savery's engine, "that with little art it can be kept in action as long as one will, and that it cannot be hurt but by ſtupidity and negligence." Harris looked on this invention of Savery's to be one of the moſt ingenious combinations that had ever been made in hydraulics, and I may add, that it is ſurpriſing that a ſeafaring gentleman, in all likelihood little uſed to the working of metals, ſhould have brought his invention to the perfection he left it in.

* It is ſeen that Papin is the firſt that thought of a piſton to be applied in fire machinery, though it was but in a tube about two inches diameter, yet it ſhewed the utility of piſtons, which the ancient Greeks knew as well as the moderns, being one of the moſt ſimple inventions in hydraulics, as may be ſeen in Vitruvius, ſpeaking of the pumps of Cteſibus for the forcing water to a great height.

In

In the memoirs of the Academy of Sciences at Paris, page 112, 1699, is to be ſeen an account of Mr. Amonton's, who ſhewed the drawing of an engine, in which fire heated air, which was to force up water on a wheel, to make it turn. We ſuppoſe he underſtood his theory; but to me it appears impracticable, though it muſt be ſaid that Monſieur Amonton's notions are ingenious and uſeful for thoſe who deſire to know to what degree the expanſion of air, for procuring power, can be brought. Amonton's rotatory engine, 1699.

There is alſo ſeen in the ſame Academy's works, "Mr. D'Aleſme's propoſal to raiſe water by means of ſteam, proceeding from hot water; in a machine (a ſort of æolipile) in which the elaſticity of the ſteam makes the water ſpout to a great height, ſo ſtrong is that expanſive power." Auguſt 5, 1705. D'Aleſme 1705

In 1707, the famous Papin publiſhed, at Caſſel, a treatiſe on a new machine for raiſing water by means of fire; he thought his manner was better than Savery's. He ſays, "All the world ſhall know that it is the Landgrave of Heſſe to whom this invention is due, and by him it was brought to the perfection it is in, though many obſtacles were found in the execution." Papin, 1707.

I cannot help remarking, what a pity it is that ſuch an ingenious man as Papin ſhould take ſo much pains to acquire the reputation of being the firſt in the invention of fire-machinery. To appear leſs partial, he takes from his own merit to flatter the landgrave, who certainly did not underſtand thoſe matters ſo well as he: but, notwithſtanding the conſideration that is due to a ſovereign and to the celebrated Papin, I make bold to ſay, that this engine was far from having the merit of Savery's; and I can ſay more, experience has made me believe that no man has ſeen the doctor's engine work a couple of hours, as may be conceived from the difficulty he found in making ſteam, and encreaſing its elaſtic power in his receiver. Papin has nevertheleſs given us demonſtration in upwards of forty pages of calculations, which prove his underſtanding and learning. This machine is in my academical works.

In 1710, Thomas Newcomen, ironmonger, and John Cawley, glaſier, at Dartmouth, contrived to work a piſton in a cylinder, by the weight of the atmoſphere only, when they had formed a vacuum in their veſſel; but it took them much Newcomen and Cawley's atmoſpheric engine, with a piſton, 1710.

much time to bring ſuch a complication of apparatus to perfection, for which they were criticiſed more out of jealouſy than reaſon; however, they brought their engine to perform by hand as well as poſſible for that time, much to their honour, as well as to that of a country boy whoſe name was Humphry Potter, and who, to give himſelf time to play, contrived tackles to make the engine work itſelf.

The bringing this engine to perform as it did, proves that, when nature is ingenious, it finds out the way to come to its ends without the humdrum pedantry of ſchools. An ironmonger, a glaſier, and a country boy, brought to perfection the lever fire-engine, which all the univerſities and academies of Europe had not the leaſt notion of before theſe ingenious mortals appeared. This diſcovery has not been improved but in acceſſaries, and by no means as to principles, as theſe are more ſimple, and can work with leſs fuel than thoſe ſo much vaunted, if care is taken in making them.

As ſoon as theſe happy inventions came to light, Beighton, another ingenious man, ſtept forth and made tables for the dimenſions of veſſels, in proportion to the weight of the atmoſphere, which proportions are followed to this day. This has made the world believe that the whole art of fire-machinery is concentered in Newcomen and Cawley's lever-engine.

Deſaguliers, 1718, and Moura, their improvements.

In 1718, Doctor Deſaguliers made an amendment (according to him) on Savery's engine; but his method was not followed, as it cauſed the death of the manager; which muſt often happen when the dimenſions are in the manner he contrived them.

After this engine, Mr. Moura, a Portugueſe gentleman, invented ſome geers to make an engine, on Savery's principles, to work itſelf: they were ingenious, but too complicated to be put in practice.

The making a fire-engine for York-buildings gave an opportunity to improve as to the manual part, and to be leſs expenſive.

Large cylinders,

Experience in making fire-engines has brought Mr. Darby, of Colebrook-dale, to make the cylinders large enough to have a vacuum in them, equal to the weight of 80,000lb. of atmoſphere, ten and fifteen times in a minute. This perfection of workmanſhip has procured to England a great ex-

portation of cylinders, pipes, &c. to many parts of Europe and America. At Liege, and in the Netherlands, they uſed to have their cylinders from England; but, being very expenſive from the duties and carriage, &c. the induſtrious Poſſon, at Liege, with his ſmall foundry, found means to make cylinders of three pieces ſcrewed together, which he bored, and made them ſerve as well as if they were of one piece. I have ſeen one work as well as can be deſired.

one of pieces ſcrewed together.

In 1752, Monſieur Rivatz, a Swiſs gentleman, made a fire-engine on Savery's principles for the Chevalier Nugue, at *Nogent ſur Marne*, two leagues from Paris; the apparatus to make the engine work itſelf was of his invention, and performed very well; his boiler, however, being out of proportion, the man who attended the work was much hurt by the exploſion of the ſteam, which diſcredited the machine, though well contrived in every part except the boiler.

Rivatz, 1752.

☞ *Next follows the Account of the Author's own Works.*

XXII.

Obſervations and Communications on the Dry Rot in Timber, made to the Society for Encouragement of Arts *.

THE miſchief ariſing in buildings from that decay of the timber and wood-work, known in general by the name of the Dry Rot, has been, and yet continues ſo great, as to demand every attention for its prevention. In the XIIth Volume of the Tranſactions of the Society of Arts, publiſhed in the year 1794, will be found ſome valuable facts, furniſhed by Robert Batſon, Eſq. of Limehouſe, reſpecting the methods he took to prevent this evil, in one of his rooms greatly affected by it. The plan he adopted was, to charr the ends of his timbers, to take away the infected earth to the depth of two feet, and to fill up that ſpace with anchor-ſmiths' aſhes, or aſhes from a foundry, before his flooring-boards were laid. On the 15th of May, 1794, which was upwards of ſix years after the flooring was laid, as above mentioned, a minute examination of the boards, wainſcot, and timbers, was made in the preſence of a Com-

Introductory remarks by the Society of Arts.

Robert Batſon, Eſq. his method of curing the dry rot.

* Extracted from their Tranſactions, Vol. XXI.

mittee

mittee of the Society, and they were all found entirely free from any appearance of the Rot. To inveſtigate the matter more fully, a further inquiry has been made in June, 1803, and an anſwer received, that there has been yet no appearance of the Dry Rot there; the Society, therefore, think it may be of conſequence to notice the fact, and to inſert, in the preſent Volume, ſome other papers with which they have been favoured upon the ſubject. They contain many hints deſerving public attention, and which will doubtleſs tend to check the progreſs of this evil.

Letter from BENJ. JOHNSON, *Eſq. on the ſame Subject.*

To CHARLES TAYLOR, Eſq.

SIR,

Account of a pew very much affected by the dry rot.

SOME time between 1771 and 1773, I went, at the requeſt of a friend, to the Chapel at the Lock-Hoſpital, through curioſity, to examine a pew there, that had frequently been repaired for damages by the dry rot.

It is a plant.

After a cloſe inveſtigation, we found that it was the operation of a plant, whoſe leaf reſembled that of the vine. Wherever it had touched, the effect of its poiſonous quality got through the wood to the paint, which I have ſeen a mere ſkin. I pro-

Deſtroyed by covering the ground.

poſed to cover the floor with bricks, laid in mortar, which was accordingly done. I called twice ſince, the laſt time about ſeven years ago; and have reaſon to think that it had never appeared again.

Dry rot at Mark-Hall.

The next opportunity of examining it carefully wat at Mark-Hall, in Eſſex, the ſeat of Mr. Montague Burgoyne. In a parlour there were three pillars of about ten inches in diameter, the outwood of which was between two and three inches thick. Two of them were eaten through in leſs than ſeven years, from the baſes, about two feet upward, within the hollow, and were as rotten as if it had been the effect of a

The plant found.

hundred years ſtanding. Mr. Montague Burgoyne's gardener was a botaniſt: we found the plant where I directed him to ſearch for it; and he ſaid it was the *Boletus Lachrymans.**

At

* Some authors call it a paraſitical plant; and it is ſometimes to be found with the willow and ſallow tribe, but this is not to the pu rpoſe.

At another time, I ſaw it in a houſe at Whitehall, built by Sir John Vanbrugh, whoſe nephew then lived in it. The houſe is, I think, only two ſtories high; the plant had aſcended to the upper ſtory, committing devaſtation on the wainſcot all the way. It will deſtroy half-inch deal wainſcoting in a year. Another inſtance at Whitehall,

I have had it twice in houſes I inhabited, one in Suffolk, and the other in Glouceſterſhire. I bore with the firſt; in the other caſe, I undertook, and did ſtop it effectually. and other places.

The cauſe is from the floor being laid on the earth, which has been, where I have obſerved, of a gravelly or ſandy loam. The moiſture from a water-courſe at hand, or a North aſpect, where the outer wall ſtands in a garden-bed, ſo that the rain percolates, are great encouragers: it requires moiſture. It is cauſed by communication with moiſt earth;

It never riſes in the middle of the floor; becauſe, if the ſeed were there, it could not germinate for want of air; but it is eaſy to ſuppoſe, that after the floor is ſhrunk, an air may be created between that, and the vacancy between the wainscot and outer wall ſufficient for the purpoſes of vegetation. with acceſs of air.

I ſaw an inſtance, laſt ſummer, in the houſe of a friend, a ſtudent in botany. He was ſurpriſed when I told him, it was a viſit from a plant; but ſo it proved, and always is, and ever was ſo; nor does it originate from any other cauſe.

In my own caſe, I removed the original ſoil near the part affected, and ſupplied its place with ſand. I then placed pieces of tile; on thoſe I laid mortar, and tiles over them, puſhing them under the wainſcot, ſo that it had no communication with the joiſts or floor. Pillars, in like manner, ſhould be kept from the earth. Proceſs of cure.

In laying a floor upon the ground, I ſhould take away the earth for a foot in breadth, and four inches in depth, all round the walls, and place the ends of the joiſts in mortar, covering them with tiles preſſed under the floor and wainscot, quite to the outward wall. Iron or tin plates would do, but are not ſo cheap as mortar and tiles.

purpoſe. Till within a few months I have never been without ſome leaves of the plant. For many years they appear exhauſted and dead, and ſoon crumble into duſt; but I ſuſpect that freſh wood attracts a freſh growth from the root.

This

The plant does not run except along wood.

This plant has no adhefive powers, but in contact with wood. If it could pafs over brick or mortar, it might be feen to fpring from the cellars, and infect half the houfes in the kingdom.

In fhort, the wainfcot is to be kept free from contact with the joifts and floor ; and I believe it cannot be better effected than I have defcribed.

I am, SIR,

Your obedient Servant,

Dec. 21, 1799. BENJAMIN JOHNSON.

To the Secretary.

(To be continued.)

SCIENTIFIC NEWS.

*Society of Agriculture, Commerce, and Arts, at Boulogne-fur-Mer.**

THE following queftions propofed by this fociety are noticed as of general and not local import.

What are the nature of marles, their fpecies, and the beft modes of ufing them ?

1. It is generally acknowledged, that marling is one of the moft important means of improving land; accordingly the fociety will beftow a prize on the memoir, in which the nature of marles, their various fpecies, and the moft advantageous mode of ufing them, according to the difference of foils, fhall be beft unfolded.

Their external characters neceffary to be known.

The author will be particularly careful to point out to farmers external characters, that will enable them eafily to diftinguifh each fpecies of marle.

Sheep liable to the vertigo or dunt.
In France, 900,000, or 950,000, that is 3 or 4 in 100 die of it yearly.

2. Sheep are fubject to a diforder known by the name of *vertigo*, or the *dunt*. The number that die of this diforder may be eftimated without exaggeration, at three or four in a hundred annually. If then there be about thirty millions in France, we may reckon, that nine hundred, or nine hundred and fifty thoufand are thus loft every year. It is occafioned by a hydatid formed in the head of the fheep. This hydatid

* *Magazin Encyclopedique*, No. 3. June, 1803. p. 401.

contains

contains an extremely limpid water, and a white ſubſtance, divided as if into grains, which have been found by the microſcope, to be real worms. It appears, that to cure this diſorder, the head of the ſheep muſt neceſſarily be opened, and the hydatid extricated: but this operation has hitherto appeared ſo hazardous, and has been attended with ſo little ſucceſs, even in the veterinary ſchool at Alfort, that the Society has deemed it highly important to the ſcience of agriculture, to offer a prize to the perſon who ſhall point out the beſt methods of treating the vertigo of ſheep, and curing them perfectly of this diſorder. He will take care that his method be ſupported by experiments made with accuracy, and confirmed by unqueſtionable teſtimony.

Cauſed by a hydatid in the head.

Its extraction dangerous.

What is the beſt method of treating it?

The prizes will conſiſt of medals, which the ſociety will deliver at the public meeting, on the 28th of April, 1804.

Cryſtallization of Lime by Tromſdorff, denied by Berthollet.

IN the firſt volume of the octavo ſeries of this Journal, p. 302, is inſerted "A method of cryſtallizing lime, by Profeſſor Tromſdorff," tranſlated from the Journal der Pharmacie, Vol. IX. part I. p. 108. The ſame proceſs is alſo in Van Mons's Chemical Journal, No. II. According to which, muriate of lime is to be boiled with one fourth or even leſs of cauſtic lime, and the fluid is to be concentrated. Afterwards, by ſlow evaporation, long ſlender cryſtals of *cauſtic lime* are obtained, which are to be freed from the adhering muriate by ablution in alcohol.

Error reſpecting the cryſtallization of lime.

This reſult is denied by Berthollet, who, in his *Eſſai du Statique Chimique, Vol. I. p.* 350, ſays, "I have repeated this experiment, and have aſcertained that theſe cryſtals were *not lime*, as has been announced, but *muriate of lime with an exceſs of lime.*" He continues, "if theſe cryſtals be treated with water, other proportions are eſtabliſhed; the part which diſſolves is the muriate, which retains but a ſmall exceſs of lime, and the portion which is not diſſolved retains a greater exceſs of lime; by adding water, ſucceſſive ſeparations may be obtained, and the proportions will depend on the relation of the diſſolving force to the reſiſtance of the coheſion."

Converſion

Converſion of Pieces of Cloth impregnated with a Solution of Salt into Soap of Wool, by the Action of the Galvanic Pile. By BRUGNATELLI.

Cloth wetted with ſalt and water, uſed in the galvanic pile, is converted into ſoap of wool; the muriate being decompoſed by the zinc, which is oxided, and the ſoda combining with the wool.

I HAVE examined the ſingular alteration produced in the bits of cloth wetted with a ſolution of ſalt, interpoſed between the diſks of copper and zinc arranged in the pile, and I have found, that they are at length converted entirely into ſoap of wool.

On this occaſion the muriate of ſoda is decompoſed by the zinc, that is continually undergoing oxidation, and the ſoda, ſeparated by this decompoſition, attacks the wool, and forms with it ſoap.

THE Work of Mr. Parkinſon, of Hoxton, on the organic remains of the former world, is in conſiderable forwardneſs. The firſt part on the foſſils of the vegetable kingdom, illuſtrated with coloured plates, in quarto, is propoſed to be publiſhed on the firſt of June next.

Apparatus for Cleansing Chimnies, by Mr. J. C. Hornblower.

Fig. 1.

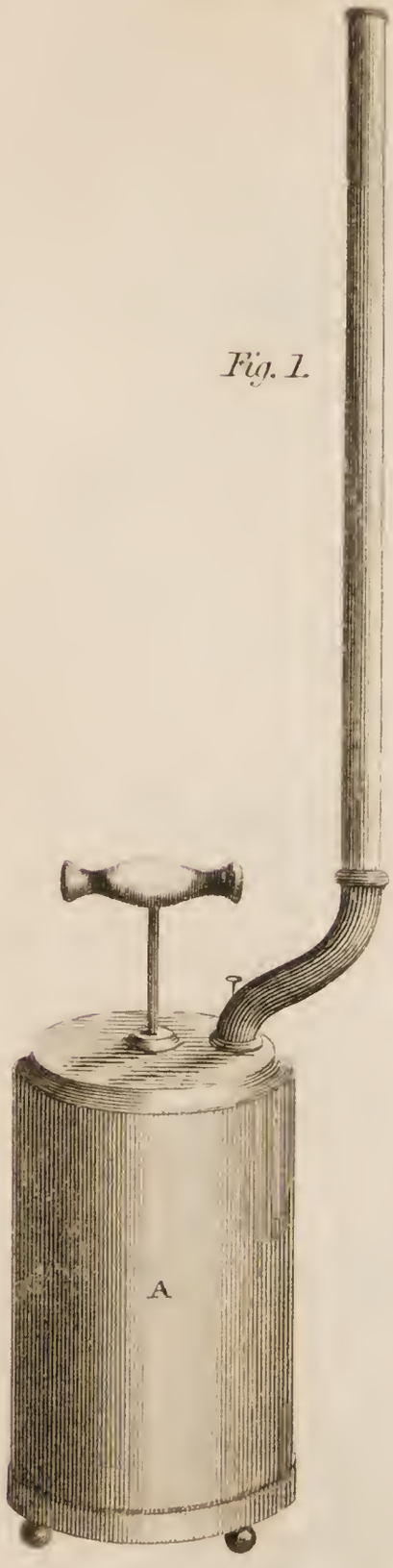
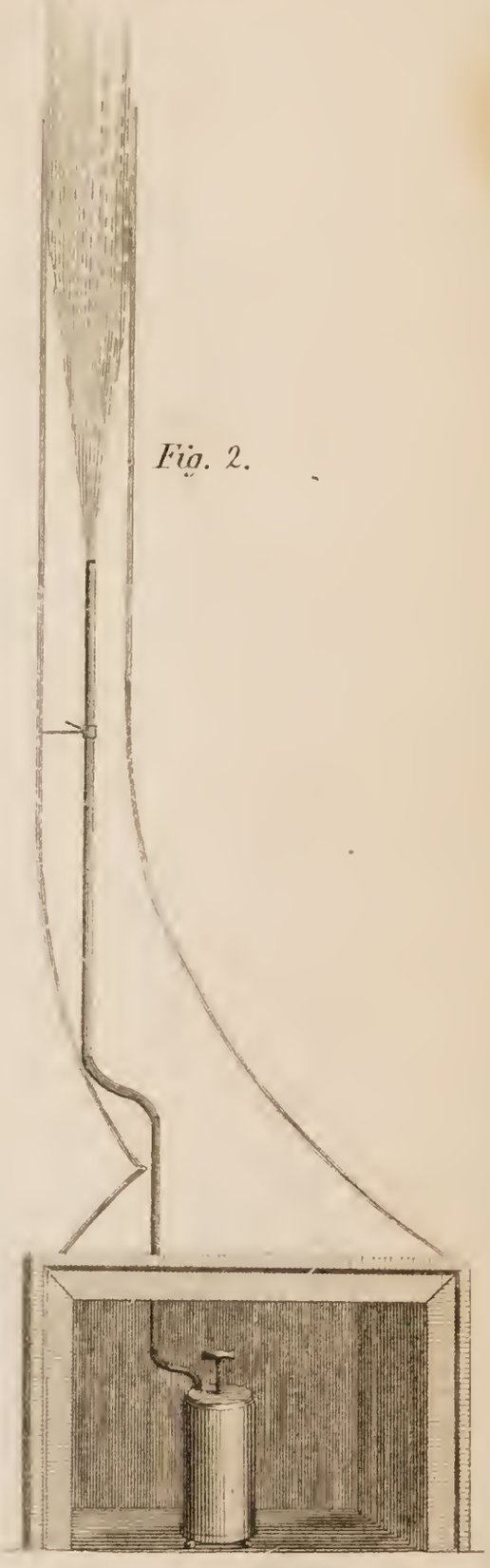


Fig. 2.



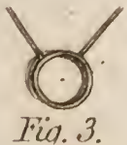


Fig. 3.

Drawn by Blunt. Engraved by Mallow.

Dr. Herschel's Investigation of the Changes observed in double Stars.

Fig. 1.

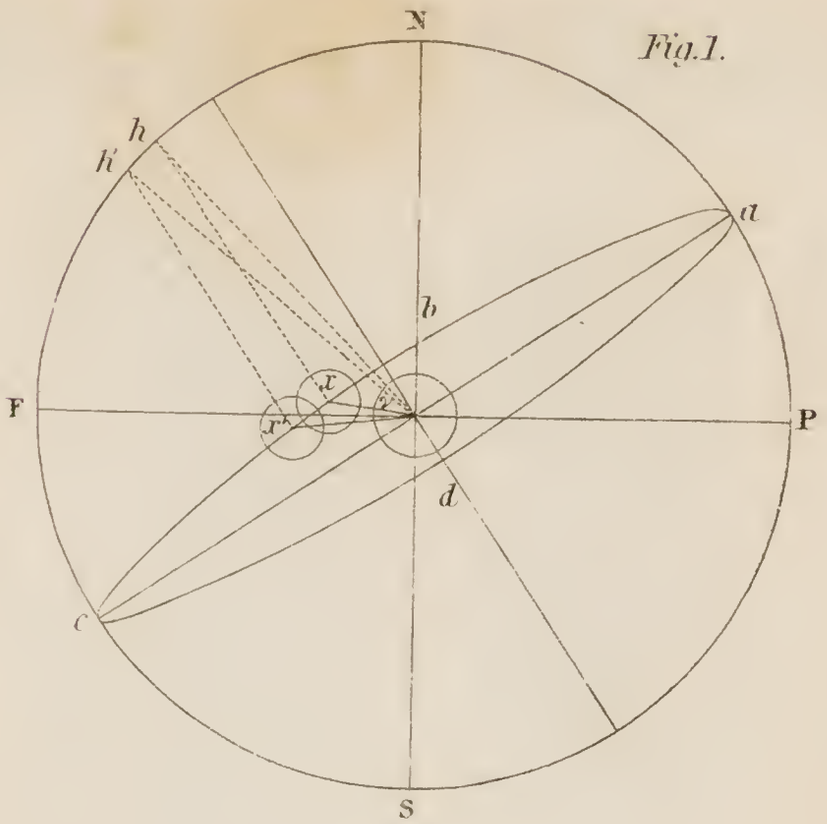
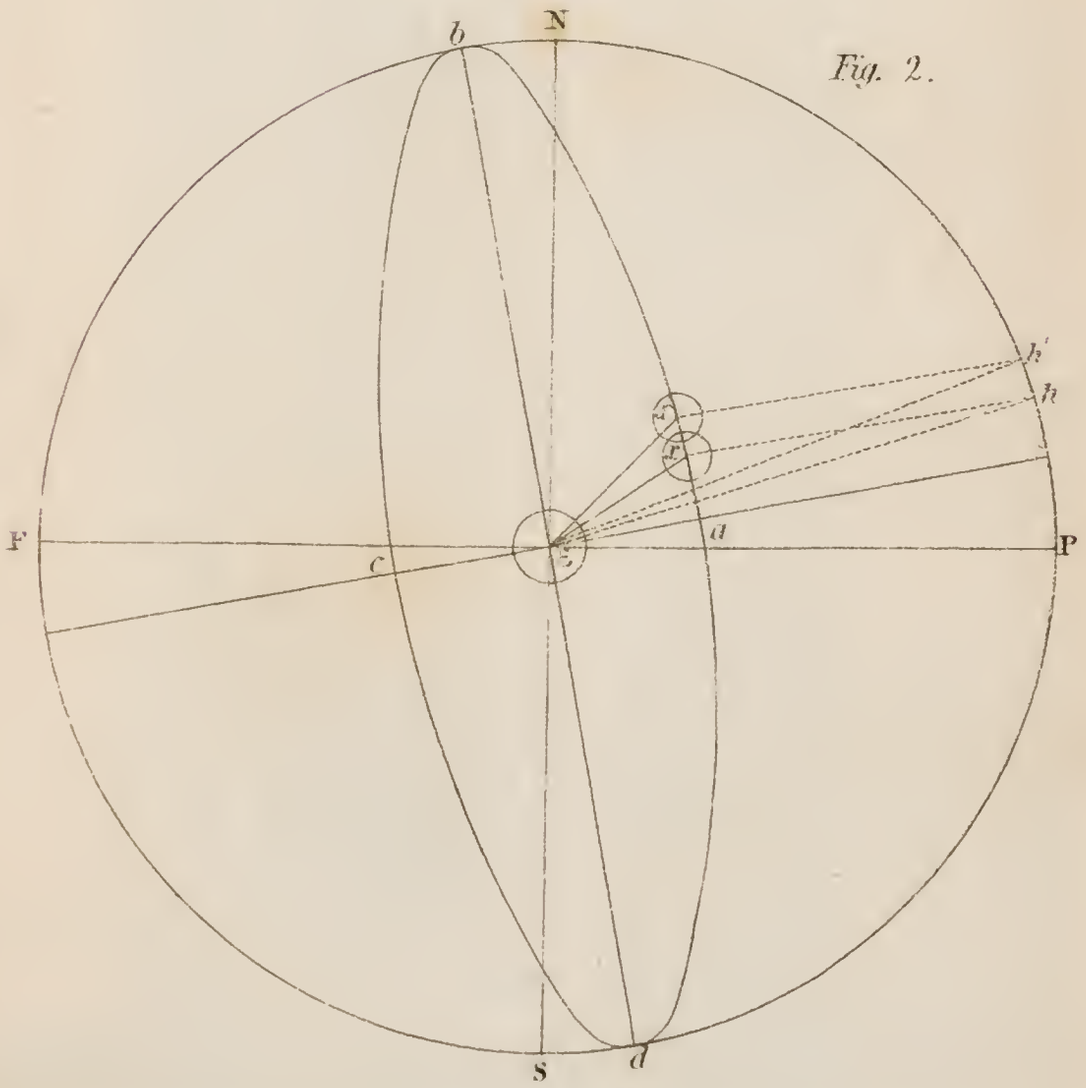


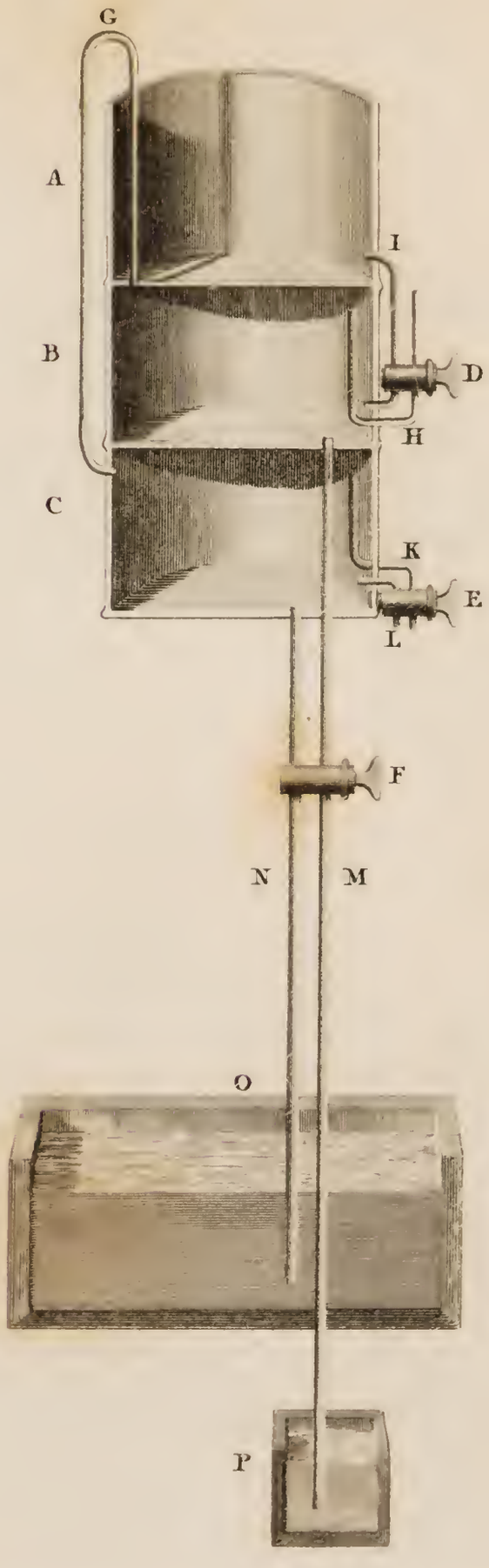
Fig. 2.



New Galvanic Apparatus for combustion of Metals.

Mathews Sc. Flagstaff Cl.

Mr. Sharples's Apparatus for raising Water by the fall of Waste Water.



Drawn by Blunt.

Engraved by Mudlow Rupsell Ost.

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